PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

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Ninety-First Session.

Monday, 2d March 1874.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On the Parallel Roads of Glen Roy. By the Rev. Thomas Brown, F.R.S.E.

(*Abstract.*)

After describing the general appearance of these terraces, the author referred to the discussions which had taken place as to their formation. More than fifty years ago it was conclusively proved by Dr Macculloch* and Sir Thomas Dick Lauder,† that these parallel roads are the margins of ancient lakes, and since then the question has been whether these were freshwater or sea lochs. Mr Darwin, Dr Robert Chambers, Professor Nichol, and others, have contended that they were marine; while Agassiz, Dr Milne Home, Mr Jamieson, and others, have maintained that they were freshwater.

As the problem is confessedly one of some difficulty, it seemed desirable to obtain, if possible, the evidence of fossils. It has been ascertained, indeed, that the deposits contain no shells nor similar remains, and Mr Darwin has suggested that they may have been destroyed by the carbonic acid gas absorbed by the rain-water which for ages has percolated through the beds. This, however, could not have taken place with the valves of diatoms, which are siliceous, and on which, therefore, carbonic acid could produce no effect. They have this further advantage, that the marine and freshwater species each keep to their own distinct localities, and if any such fossils, therefore, could be found in the parallel roads, they might give important assistance in deciding between the marine and freshwater theories.

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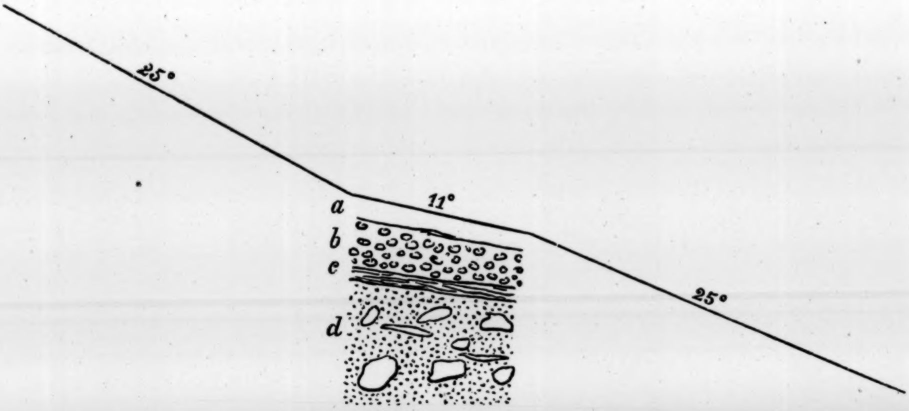
* Geol. Trans, ser. 1, vol. iv.

† Edin. Roy. Soc. Trans., vol. ix.

Accordingly, in the autumn of 1872, and again in 1873, an attempt was made to obtain fossil diatoms from these deposits. It was found that in Glen Roy there were a good many points where the parallel roads were cut through and laid open. Four of these sections were examined with some care, and one was especially fixed on lying high and dry on the hill side, where the internal structure of the lowest terrace was distinctly shown. The object was to obtain specimens of diatoms which may have been alive when the terraces were formed, and which were then included in the deposits; but to secure this, various precautions were necessary. The nature of the different beds composing these terraces will be



a,	Humus—peaty, . . .	8 in. to 1 foot.
b,	Stones with clay, . . .	2 to 3 feet.
c,	Finely stratified sand and clay,	3 in. to 8 in.
d,	Clay with boulders indistinctly stratified, with thin irregular courses of sand,* . . .	about 20 feet going down to the rock.

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* It may be a question whether this bed *d* really belongs to the time of the lowest shelf, or whether it is not some anterior formation—the sloping bottom of the lake, perhaps, at some earlier time.

understood from the preceding diagram representing the section just referred to.

In searching for diatoms, it was necessary to avoid the bed *a*, which has been formed since the time of the parallel roads. It was thought safer also to throw out of view the bed *b*, the upper surface of which is in contact with *a*. Attention was therefore confined to the beds *c* and *d*. The outside weathered portion of the bed was in each case removed, and part of the internal contents of the bed cut cleanly out—that from *d* being about 10 feet below the surface. The material thus obtained was washed in distilled water and microscopically examined. It was found that the search required much patience. Diatoms were ascertained to be present scattered very rarely through the material, but at last a series of specimens were got. These were sent to Professor Dickie, of Aberdeen, one of our highest authorities in this department of natural history, and the following species were determined by him:—

Pinnularia viridis.
Himantidium undulatum.
Diatoma vulgare.
Surirella panduriformis?

Of these the first three were got from bed *c*, and the first two and the last from bed *d*.

Now these are all freshwater species, and their evidence is strengthened by the fact that there is the entire absence of any marine diatom or other organism. This would indicate that it was an old freshwater lake which had these parallel roads for its margins. Freshwater diatoms might, indeed, have been brought down into it even if it had been a sea loch, but the important fact is, that while freshwater species are found, it has been impossible to detect a single trace of anything marine.

It is indeed true that it is only a single locality which has been searched in this way, and it would be going too far to hold the results as at once conclusive. Enough, however, has been done to show that this method of approaching the solution of the problem deserves to be followed out. Search should be made at other points along these parallel roads where they are laid open. They have been a kind of battle-field fought over by rival theorists for the last fifty years, and it will be strange if all the time multitudes of witnesses have been lying shut up in the deposits, only waiting to be called into court to give decisive evidence. So far as the investigation has gone, it is in favour of the Freshwater Theory.

2. Note on the Perception of Musical Sounds.

By John G. M'Kendrick, M.D.

Certain individuals appear to be incapable of appreciating musical sounds. They cannot distinguish one melody from another; and if by many repetitions of the melody in their hearing, they at last appear to know it, the addition of one or more of the parts of the harmony again renders the music unrecognisable to them. The question naturally arises, Is this defect owing to any peculiarity in the structure of the internal ear of persons so constituted which prevents them hearing certain sounds, or is it to be referred to the condition of the brain? On the other hand, many have what is termed a "fine ear," by which we understand the faculty of appreciating, remembering, and, in some cases, of successfully imitating musical sounds. Have those individuals the organ of hearing more delicately developed?

This physiological problem does not, in the present state of our knowledge of the minute structure of the organ of hearing in man, permit of being examined histologically. We would not probably find any appreciable histological difference between the internal structure of the ear of a genius in music and that of a person who could not distinguish one melody from another. So far as this method of inquiry is concerned, differences may exist, but the minute size of the ultimate recipients of sound-waves, and the vagueness of our present knowledge of the number of these in the depths of the cochlea, would prevent any one from noticing those differences.

It, therefore, occurred to me to examine this question by testing experimentally whether those individuals who profess to be unable to know music were incapable of hearing certain musical sounds, limited as regards pitch, within the extreme keys on the key-board of a piano. I have examined ten cases of this kind.

In a musical sound three elements have to be considered,—1*st*, loudness or intensity, which depends on the extent of vibration;

2*d*, pitch, determined by rate of vibration; and, 3*d*, quality, which depends on the orders, numbers, and relative intensities of the simple tones into which it can be resolved.

Up to the present point of this inquiry I have devoted attention chiefly to the element of quality. The apparatus I have employed was made by Georg Appunn of Hanau. It is a long wooden box inclosing a row of vibrating tongues or free reeds, which can be thrown into action by propelling air into the box by means of a bellows. The note produced by the longest reed, No. 1, is that obtained by a vibrating cord, of a certain length, thickness, and tension, as in a monochord, and corresponds to C^2, having 32 vibrations per second. On dividing the cord into 2, 3, 4, 5, 6, &c. equal segments, each segment, when caused to vibrate, will produce a note composed of 2, 3, 4, 5, or 6 times the number of vibrations in No. 1. This apparatus is capable of producing 64 tones, a larger number than are included within the key-board of a piano. The names and number of vibrations per second of these tones in this apparatus is as follows:—

No.			
1.	C^2	32,	Fundamental tone.
2.	C^1	64,	Octave.
3.	G^1	96,	Fifth above No. 2.
4.	C^0	128,	Fourth above No. 3.
5.	e^0	160,	Major third above No. 4.
6.	G^0	192,	Minor third above No. 5.
7.	C^0	224	
8.	C^1	256	
9.	D^1	288	
10.	e^1	320	
11.	F^1+	352	
12.	G^1	384	
13.	a^1-	416	
14.	b^1	448	
15.	h^1	480	
16.	C^2	512	
17.	Des^2	544	
18.	D^2	579	
19.	Es^2-	608	
20.	e^2	640	
21.	f^2	672	
22.	F^2+	704	
23.	Fis^2	736	
24.	G^2	768	
25.	gis^2-	800	
26.	a^2-	832	
27.	a^2	864	
28.	b^2	896	
29.	Ais^2	928	
30.	h^2	960	
31.	H^2	992	
32.	C^3	1024	
33.	C^3+	1056	
34.	Des^3+	1088	
35.	d^3-	1120	
36.	D^3	1152	
37.	D^3+	1184	
38.	Es^3-	1216	
39.	e^3-	1248	
40.	e^3	1280	
41.	E^3+	1312	
42.	f^3	1344	

No.			No.		
43.	f^3+	1376	54.	A^3	1728
44.	F^3+	1408	55.	A^3+	1760
45.	Fis^3	1440	56.	b^3	1792
46.	Fis^3+	1472	57.	B^3	1824
47.	g^3	1504	58.	Ais^3	1856
48.	G^3	1536	59.	Ais^3+	1888
49.	As^3−	1568	60.	h^3	1920
50.	Gis^3−	1600	61.	H^3+	1952
51.	As^3	1632	62.	H^3++	1984
52.	a^3−	1664	63.	c^4−	2016
53.	a^3	1696	64.	C^4	2038

I have also a series of 64 resonators, tuned to these 64 tones, and having corresponding numbers. When tone No. 12 on the overtone apparatus is sounded, and the narrow end of resonator No. 12 is placed in the ear, the instrument sings into the ear of the observer with great intensity. I have thus in the group of resonators an apparatus for analysing any compound musical note into its constituent tones; and, in the overtone apparatus, I have a means of checking the sensation of the listener by sounding, with much greater intensity, the tone corresponding to the resonator by which he heard any particular tone in a compound note. The method I adopted was,—1*st*, to strike a note on the piano, which, of course, consisted of a fundamental tone, and of certain overtones; 2*d*, to allow the person whose ear was being examined to listen with the various resonators until he selected one by which he heard a tone (one existing in the note, and strengthened in intensity by the resonator); 3*d*, after the listener had satisfied himself that he clearly heard the overtone ringing in his ear, the note on the piano was arrested, the stop of the overtone apparatus corresponding to the overtone was withdrawn, so as to sound the overtone, and the listener had to decide whether or not this was the same sound as the one he heard when listening to the musical note. The result in the ten cases I examined was as follows:—In nine of the cases the overtone was readily perceived; and in the tenth, the lower overtones of the series were observed directly, whereas the higher overtones were not noticed by means of the resonator, but were clearly observed when those overtones were sounded on the overtone apparatus. This individual asserted he had often noticed he was deaf to very highly-pitched sounds which other people said they heard.

These results indicate that, so far as the structure of the ear is concerned, those individuals who are said not to know one note from another, are equally capable, by the use of resonators, of analysing a compound musical note—that is, of hearing the various tones of which it is composed—with those who have a good ear. Physiologically, they seem to be capable of splitting up, unconsciously, the compound vibration into the simple vibrations, the rates of which are once, twice, or thrice that of the fundamental note.

The next point which I examined was regarding the perception by persons having no musical ear of difference and summation tones, which, as is well known, play an important part in the theory of concord and discord.

If, on the overtone apparatus, two tones of different pitch are sounded, a third and deeper tone may be frequently observed. These tones were first discovered by a German organist, Andreas Sorge, in 1740. For example, if 2 : 3, or 3 : 4, or 6 : 7, or 7 : 8, &c., are sounded, a third and deeper tone may be perceived by the use of a proper resonator, which will be always found to be $C_2 = 1$; that is, this combination tone is produced by 32 vibrations per second, the difference between the respective vibration numbers of the tones 2 : 3, or 3 : 4, or 6 : 7, &c. I have found that the difference tone heard with greatest distinctness corresponds to one produced by 128 vibrations per second. For example, on sounding 16 : 20, or 24 or 28, or 32 and 36, with resonator No. 4, I can distinctly hear the tone corresponding to No. 4 = 128 vibrations per second in each case. I have found no marked difference between non-musical and musical individuals in the perception of difference tones, except as regards intensity. I had the opportunity of examining two persons of marked musical ability, who could distinguish, by great attention, without the aid of resonators, difference tones to the 6th of the series, and who could observe difference tones, 2, 3, and 4, with comparative ease. Non-musical persons did not observe these difference tones without the use of resonators to add to their intensity; and, in one case, the person could not hear them at all. In addition to these primary difference tones, I have met with only one individual who could hear what are termed secondary and tertiary difference tones, and he could not hear these without apparently a strong effort of attention. They were as follows:—On sounding 16, $C^2 = 512$ vibrations per second, and 20, $e^2 = 640$, he heard $C^0 = 128$, that is, $20 - 16 = 4$. By using resonator No. 12, he heard $12 = 384$, that is $16 - 4 = 12$; and on using resonator No. 8, he heard very feebly 8, that is $12 - 4 = 8$. When C^2 and e^2, that is tones corresponding to 512 and 640 vibrations per second, were sounded in this person's ears, he heard other three tones with the use of resonators, namely, those produced by 128, 256, and 384 vibrations per second.

But when two tones are sounded, in addition to a tone produced by a vibration number equal to the difference between the vibration number of the two, another tone is produced, the vibration number of which is equal to the sum of the vibration numbers of the two primaries. This tone is called a summation tone. For example, on sounding $4 = 128$ and $6 = 192$ vibrations per second, by using resonator No. 10 = a tone having 320 vibrations per second may be distinctly heard. Thus, 2 : 3, and 3 : 4, and 5 : 9, will produce sounds heard by resonators Nos. 5, 7, and 14, respectively. I have found that non-musical people can hear these summation tones with great distinctness if increased by resonators. They can hear the lower order of summation tones much more easily than the higher order. For example, all could hear the summation tone 2 (64) : 3 (96) = 5 (160) –, or 4 (128) : 6 (192) = 10 (320); but only four out of the ten could hear 7 (224) + 8 (256) = 15 (480), and 8 (256) + 9 (288) = 17 (544). Only one out of the ten could hear 30 (960) + 28 (896) = 58 (1856), and none could hear 32 (1024) + 30 (960) = 62 (1984). I observed also that they could hear the higher summation tones only when the intensity was increased to as great an extent as possible. The two musical persons examined were able to hear all these sounds with ease, with even diminished intensity.

According to Helmholtz, there are secondary and tertiary summation tones, which spring from combinations of the primary summation tones with its elements. Thus, 3 (96) : 5 (160), with the overtone apparatus, give 8 (256); 3 (96) and 8 (256) give 11 (352); 5 (160) and 8 (256) and give 13 (416). Therefore, when 3 (96) and 5 (160) are sounded, according to this statement, the listener with resonators may hear 3 (96), 5 (160), 8 (256), 11 (352), and 14 (448). I have examined this and various other combinations. I can with my own ears hear, by using the appropriate resonator, the primary combination series quite distinctly, but no farther. The secondary tones I have never heard. Eight out of the ten non-musical people I have examined have heard the primary series; the other two said they thought they could hear the second. The two musical persons asserted they could hear the tones distinctly.

If then "the presence of overtones confers on music its most characteristic charms," as stated by Sedley Taylor,* it appears to me that non-musical persons, when aided by resonators, are as capable as musical persons of recognising the existence of certain of these overtones. The difference between the two classes of listeners is either—(1), that the intensity of the overtone requires to be greater to be appreciated by a non-musical than by a musical person; or (2), that musical persons, by previous education of the sense, are better able to appreciate distinctions of sound. Non-musical persons seem to be incapable of noticing the existence of the higher overtones, which are, of course, much less intense than the lower overtones. They are incapable of observing the difference and summation tones having high vibration numbers. Thus, so far as the mere perception of musical sounds, and of those secondary vibrations, which produce overtones, and give quality to the fundamental tone, or duad, or triad, &c., is concerned, non-musical persons are affected by the vibrations just as musical persons are affected. The only difference I have noticed between the two is that of intensity. A musical person hears tones of low intensity, such as the higher overtones, quickly, and apparently without difficulty; whereas, a person who is non-musical hears the lower overtones, but he cannot hear the upper at all, even with the aid of a resonator. The question of intensity of tones and overtones I have still under experimental inquiry. These researches indicate that in the sense of hearing there is no state analogous to that of colour-blindness in the eye.

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* Sound and Music: A Non-Mathematical Treatise on the Physical Constitution of Musical Sounds and Harmony, &c. By Sedley Taylor, M.A., &c. London, 1873.

3. On the Establishment of the Elementary Principles of Quaternions on an Analytical Basis. By G. Plarr, Esq. Communicated by Professor Tait.

4. Preliminary Note "On a New Method of obtaining very perfect Vacua." By Professor P. G. Tait and Mr James Dewar.

Professor Andrews, in the "Philosophical Magazine" for 1852, recalled the attention of physicists to the method originally devised by Davy of making a vacuum so perfect, that the residual gas exercised no appreciable pressure as registered by the depression of a barometric column. This he effected by filling the vessel to be exhausted with carbonic acid gas, having previously inserted a cup containing a concentrated solution of caustic potash. On rapidly exhausting with an air-pump, and leaving time for the absorption of the residual carbonic acid by the caustic potash, he obtained a vacuum as perfect as a Torricellian. Andrews' method was afterwards employed by Gassiot in his well-known investigations on the passage of electricity through attenuated media. By the use of stick potash in carbonic acid vacuum tubes, he succeeded in rendering the tubes so free from any traces of gas, that the electric discharge will not pass. Caustic potash for this purpose is unsatisfactory, from the fact of its requiring to be fused before rapid absorption takes place, and also from the fact that aqueous vapour is apt to be left. This plan is besides entirely confined to carbonic acid tubes, although other chemical agents might be procured to effect absorption of traces of other gases. The method we have devised to absorb traces of gases is based on the remarkable power of absorption of cocoa-nut charcoal for gaseous bodies generally. By placing a piece of this charcoal in a glass tube, having two platinum terminals for the purpose of passing an electric discharge and exhausting with a Sprengel pump, heating the charcoal to a low red heat during the exhaustion, when the tube is sealed the vacuum is so perfect that no spark will pass with a coil giving quarter of an inch sparks in air. On now heating the charcoal with a spirit lamp, sufficient gas is given out to allow the spark to pass; on cooling, rapid reabsorption takes place, and the tube is rendered again impervious to the discharge. This operation may apparently be repeated *ad infinitum* with the same results.

Many important determinations may be effected through the employment of carbon vacua, such as the temperature at which dissociation takes place between the carbon and the dissolved gas, the time required for reabsorption, and the effect of different gases in influencing the action of the carbon. We need hardly say that this easy means of obtaining vacua will be of importance in spectroscopic observations, and we intend shortly to communicate observations in this direction.

5. Laboratory Notes. By Professor Tait.

1. *On Atmospheric Electricity.*

For some days past I have been in the habit of observing atmospheric electricity about one o'clock, with the view of ascertaining whether the concussion produced by the time-gun has, as I suspected from an experiment of ten years ago, an effect on the amount collected by the water dropper. For several successive days the atmospheric charge was small and only slowly variable, and uniformly on these occasions I found a sudden slight increase of the deflection of the electrometer (whether it was originally positive or negative) to occur simultaneously with the sound, precisely the result obtained in the single experiment of date 21st May 1864. It appears to be most probably due to mere shaking of the instruments.

On Thursday last, the 26th, during the great storm, the amount of electricity collected was so large, as in general to be beyond the range of my divided ring electrometer after a fraction of a second. I therefore connected the water dropper with a gold leaf electroscope, whose leaves were thick, and about five inches long by one broad. These leaves are made to diverge so as to touch the tinfoil coating of the case in periods often less than a quarter of a minute, indicating a potential of, roughly speaking, many thousand Grove's cells. The most curious phenomenon, however, was this, that at intervals, often not exceeding a minute, and while rain and hail were alternating, the charge of the electroscope, even on this large scale, changed from + to − and back again. It seemed, in fact, as if there were alternate changes of atmospheric potential from high + to high −, and not, as is usual in such weather, changes merely from higher to lower −. The water dropper projected 2½ feet from the wall of the College at an elevation of 44½ feet from the ground below, and discharged at an average about 2·5 cubic inches of water per minute.

2. *On the Thermo-Electric Position of Sodium.*

I owe to Mr Dewar's skill in manipulation the means of determining the line of sodium in the thermo-electric diagram. He constructed for me a long quill tube of German glass, with platinum wires inserted near the ends; exhausted it by means of a Sprengel pump, and drew in melted sodium from a bath of paraffin. Exact determinations will require considerable time, even with this excellent apparatus; but in the meantime I may state (as a first approximation) that the line of sodium is nearly parallel to that of palladium, and somewhat above it in the diagram.

The following Gentlemen were elected Fellows of the Society:—

John Anderson, M.D.
James Napier, Esq., Glasgow.
Alexander Hunter, M.D., F.R.C.S.E.

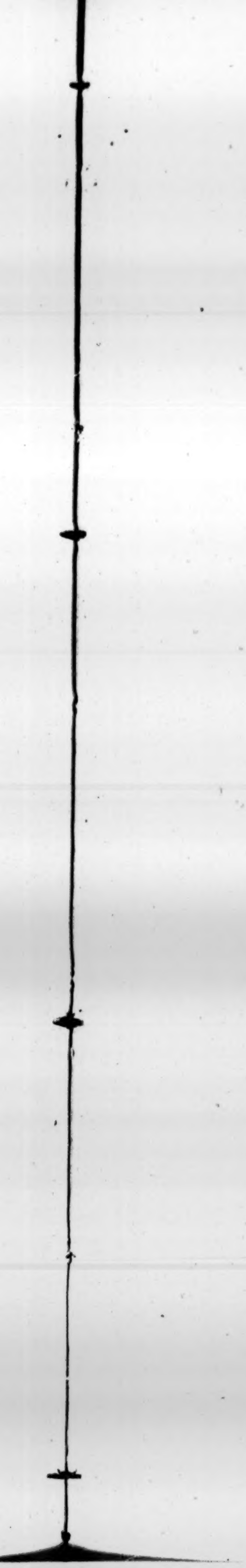
The following Gentlemen were elected Honorary Fellows, to supply the vacancies caused by the deaths of Sir John Herschel, Sir Roderick Murchison, John Stuart Mill, Hugo Von Mohl, Wilhelm Karl Haidinger, Baron Justus von Liebig, and Gustav Rose:—

1. *British Honorary.*

James Joseph Sylvester, LL.D., London.
William Hallowes Miller, LL.D., Professor of Mineralogy, Cambridge.
John Anthony Froude, London.

2. *Foreign Honorary.*

Adolphe Théodore Brongniart, Professor of Botany, Paris.
Louis Pasteur, Paris.
Wilhelm Eduard Weber, Gottingen.
Otto Torell, Professor of Zoology and Geology, Lund, and Director Geological Survey of Sweden.



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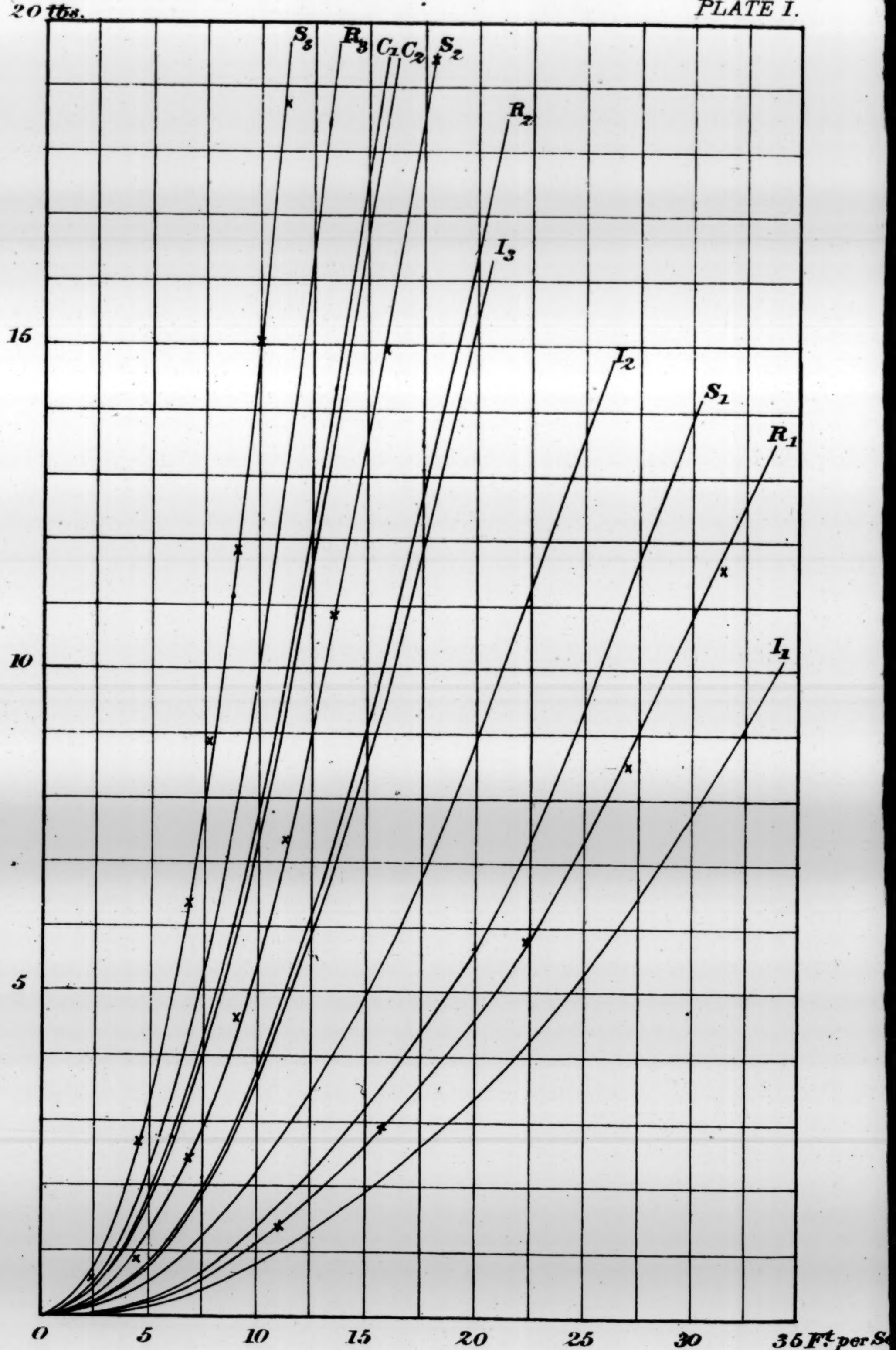
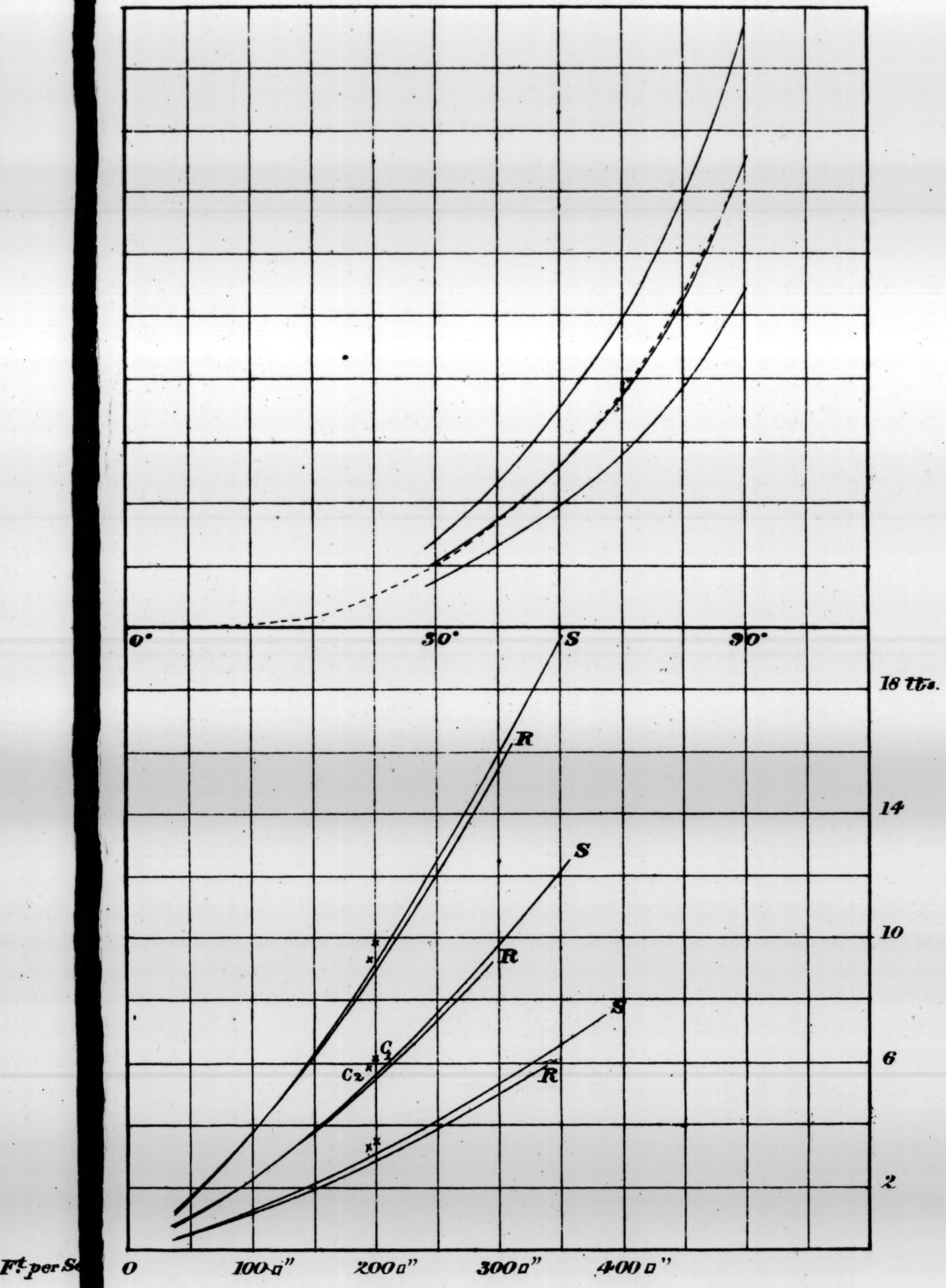
RESISTANCES IN TERMS OF VELOCITIES

PLATE I.



RESISTANCES IN TERMS OF SINES OF INCLINATIONS

PLATE II.



RESISTANCES IN TERMS OF AREAS

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Monday, 16*th March* 1874.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On the Resistance of the Air to the Motion of Fans. By James C. Fairweather, Esq. Communicated by George Forbes, Esq. (With two Plates.)

The design of this paper is to describe the results of some experiments on "the Resistance of the Air," which I made under the superintendence of Professor Forbes, in the physical laboratory of the Andersonian University, Glasgow. The apparatus employed resembles somewhat that used by M. le Chevalier de Borda, whose experiments on "the Resistance of Fluids" are recorded in the "Mémoires de l'Académie Royale des Sciences." It consists essentially of a wooden frame, which was firmly secured to the floor, and supporting a horizontal axis, at one extremity of which is fixed a wooden arm, on to which were bolted the vanes of different forms and sizes, the resistance of which it was desired to ascertain. A cylinder or barrel, about 3 inches in diameter, is fastened to the axis, whereon was wound a cord, which, being acted on by weights, gave a circular motion to the axis, and consequently to the arm, thereby carrying the vanes in the circumference of a circle of 3 feet diameter, and causing them to impinge upon the air with velocities due to the force applied. The moving force consisted of weights of from ½ lb. to 20 lbs., suspended at the end of the cord, which passed over a guide-pulley, made fast in such a position that a drop of 35 feet was obtained. For each experiment the cord was wound on to the cylinder by hand, and afterwards abandoned to the action of the weight at a beat of a second's pendulum; and the vanes thereby allowed to make a definite number of revolutions, ascertained by a distinct mark on the cord; the times of which were recorded for each observation. After a few trials the pendulum was replaced by an ordinary metronome, adjusted so as to make a vibration every half second. This instrument, from the distinctness of its beats, was found much better adapted to our purpose. The time, by this means, could be registered with perfect accuracy to one-fourth of a second of time.

The mean result of three observations was always taken with each different weight; and when there appeared to be any discrepancy, additional observations were made, in order to get a more exact average for the result. But except when the smaller weights were used this was quite unnecessary, as the results of the different observations agreed very well.

The mean result with each weight was registered, forming Table I., where, in a line with each weight, is to be found the time in seconds required for the vanes to make forty-seven revolutions.

TABLE I.

Weight on cord in lbs.	S_1 54·85 □″ Square.	R_1 41·15 □″ Round.	S_2 166·3 □″ Square.	R_2 125·8 □″ Round.	S_3 345·2 □″ Square.	R_3 264·8 □″ Round.
	″	″	″	″	″	″
1	82	73	145	140	...	...
1½	61	55	110	94	...	...
2	50	46	89	77½	138	114
3	40	36¼	70	59½	106	91
4	34	31	58	51	92	76
6	27	25	47	41½	74	61
8	23¼	21½	40½	36	64	53
10	21	19¼	37	32	56	47½
12	19	17½	34	29	52	44
14	18	16½	31½	27	48½	41
16	16¾	15½	29½	25½	45	38½
18	16	14½	28	24	43	36½
20	15	13¾	26½	22	41	34½

To ascertain the absolute resistance of the surfaces, curves similar to those on Plate I. were laid down, where the ordinates represented pressure in pounds, and the abscissæ velocities; the vanes now moving perpendicularly to their planes. In the same manner curves were laid down from Table II., which shows the weights required to give different velocities when the vanes moved in their own planes. The difference between the ordinates of a pair of curves belonging to the same vanes, the velocity being the same in both, gives the weight which, with that velocity, was required to overcome the resistance of the air. (The skin resistance is neglected as insensible). By this means the effects of inertia and friction in the apparatus are *completely eliminated.*

The equation of the curve of absolute resistances may be put in the form—

$$R = Av + Bv^2 + Cv^3 + \ \&c.$$

When R is the resistance, v the velocity, A B and C constants. Here A and C are small, but if we include A in the expression we must have C also, for A is negative. The crosses marked on Plate I. are the calculated results A and B alone being taken; in which case the formula of course fails for small values of v.

TABLE II.

Weight on cord in lbs.	S_1	R_1	S_2	R_2	S_3	R_3
1	46″	41″	47″	60″	61″	56″
2	28½	26½	30½	32½	35	33½
3	22½	21½	24	25	28½	26½
4	19¼	18½	21½	20½	24½	23½
5	17¼	16¾	...	...	21¾	20¼
6	15¾	15¼	17	17	19¾	18¼
8	...	...	15	14¾	...	...
10	...	...	13¼	13	...	...
12	...	...	12	12	...	...

In the case of the smaller surfaces, with high velocities, the resistance would appear to increase in a somewhat greater ratio. Comparing this with the results of Dr Hutton (who gives a voluminous description of his experiments in the third volume of his "Mathematical Tracts") we find them to agree. He found that "the resistance to the same surface with different velocities, is in the case of slow motions, nearly as the square of the velocity; but, gradually increasing more and more above that proportion as the velocity increases." This is rendered obvious by calculating the index of the power after his manner, and tabulating the results as annexed.

Velocity.	Resistance.	Index.
·01	10·5	...
·015	24·5	2·089
·02	43·5	2·050
·025	70·0	2·070
·03	101·0	2·121
·035	150·0	2·122
·04	195·0	2·107
		Mean, 2·093

The index of the power of the velocity is set down in the third column for the resistance due to the curve S_2. By comparing the first velocity with each of the following ones, it will be seen that the numbers in the index column slowly and gradually increase, and would doubtless continue to do so to a very great extent. The mean of these is 2·093; whence it would appear that with these velocities, the resistance to the same surface is nearly as the 2·093 power of the velocity.

The curves marked R_1, R_2, and R_3 are derived from circular plane surfaces of 41·15, 125·8, and 264·8 square inches respectively. Those marked S_1, S_2, and S_3 are from square plane surfaces of 54·85, 116·3, and 345·2 square inches respectively. The lines C_1 and C_2 are from circular concave surfaces of 199 and 192·5 square inches, and their radii of curvature are 24·″375 and 12·25 inches respectively.

All these curves were obtained by means of vanes having their plane surfaces at right angles to their plane of rotation. The curves I_1, I_2, and I_3 were derived from a plane square surface of 166·3 square inches, inclined at angles of 30°, 45°, and 60° to the plane of rotation.

The curves on the lower part of Plate II. are intended to exhibit the manner in which the resistance increases with the surface. The abscissæ of these curves represent the areas of the surfaces in square inches, and the ordinates resistances; the velocity remaining constant. It is at once seen from these curves, that the resistance does not vary directly as the surface; but increases in a somewhat greater ratio. Within the limits of these experiments, the compound ratio of the resistance to the surface rises from 1 to 1·7.

The curves in the upper part of the same plate are intended to show the manner in which the resistance varies with the inclination of the vanes to the plane of rotation. In these the abscissæ denote the sines of the angles of incidence (the angle of incidence being the angle which the vanes make with the plane of rotation), and the ordinates resistances.

The equation which satisfies these curves, is—

$$\text{Sin}^3 i = \text{R} \times \text{C}$$

where i, the angle of incidence, R, the resistance, and C, a constant.

This being shown by a dotted line, found by calculation from this formula, and which almost coincides with the curve found from the experiments. This clearly proves that the resistance varies as the cube of the sine of the angle of incidence.

The curves C_1 and C_2 and the points C_1, C_2 on the curves of areas represent the effects due to concavity of the vanes; from which we conclude, that a certain amount of concavity offers a greater resistance than the same area, and configuration of a plane surface. But, on comparing the greater with the less concave surface, there appears to be little or no difference within the limits of these experiments. This appears to be due to the manner in which the particles act upon the surface. First, in comparing the concave surface with a plane surface of the same area, we find that the concave vane offers most resistance. This may be accounted for, by imagining a certain quantity of the particles to be caught in, as it were, in front of the vanes, and consequently forming a denser medium; this extra dense medium being continually kept up in front, while the vanes are in motion. This overcrowding of space has a tendency to prevent the particles from moving past the perimeter of the vanes with the same ease, and consequently retards the apparatus.

Again, by comparing the less with the more concave, we would at first sight conclude that this was simply an amplified case of the foregoing; but here we have something to balance the extra dense medium in front, viz., the action of the particles of the convex surface behind. Their action may be said to be analogous to the action of the water closing in at a ship's stern; and, therefore, tends to impel the surface forward, and in that way diminish the effects of any resistance due to the extra concavity in front. So that, looking at the matter in this light, we should conclude that there would be a maximum resistance with a certain degree of curvature This, however, cannot be proved by the small number of observations made curved surfaces, but would be very interesting to ascertain experimentally.

2. On the Curve of Second Sines and its Variations.

By Edward Sang, Esq.

The idea of this class of curves arose during an attempt to resolve an important problem in the doctrine of wheel-work; a statement of the conditions of that problem is thus the natural introduction to the subject.

When the shape of the tooth of one wheel A has been assumed, the shape of the tooth of another wheel B, to work along with it, may be deduced by a very simple graphic process; and when these two wheels are made to turn together, the points of contact describe a certain line or *path.* In my "New General Theory of the Teeth of Wheels" it is shown that this path, and the manner of motion in it, are independent of the size of the second wheel B, and result entirely from the arbitrarily assumed form of A; that is to say, if we delineate the form of a new wheel B, to work along with A, the path and the motion of the contact point along it are the same as before; the originally assumed form A thus gives rise to a system of conjugate forms B. Not only so, but if we use any one of the wheels B as an originator, we shall obtain from it a system of conjugates A, of which our first wheel A is a member. Thus the assumption of one wheel gives rise to two conjugate systems, A and B, so related that any wheel of the one works with any wheel of the other, the contact path remaining the same for every couple. It does not, however, follow that two wheels of the same set can work together; in the arrangement of wheel-work it is important that the two systems be identic. Now, when the wheel is indefinitely enlarged, its boundary merges into a straight rack, and the rack A is necessarily a copy of the rack B; hence we come to the most important theorem in the doctrine of engrainage, that "if we assume for the outline of a straight rack any curve consisting of equal undulations, symmetrically arranged on either side of its pitch-line, all the wheels determined by it work with each other, and are reversible face to face." This theorem is general so far as geometry is concerned, but is restricted in its mechanical application by the condition of material continuity, and hence there arise some difficult and interesting problems.

Each assumed form of undulation has its peculiar path for the point of contact; this path is obtained by drawing normals PN, meeting the line of abscissæ in N, as in figs. 1 and 9, and then through some fixed point Q, technically called the *pitch-point*, drawing QP′ equal and parallel to NP.

If we suppose the point P, in fig. 1, to move steadily along the curve RXTYV, accompanied by the normal, the point N will move continuously, though not uniformly, from R to V; and from no point in the axis RSTUV can more than one normal be drawn to the curve. Such a rack would give rise to a system of wheels having only one point of contact, and so useless in machinery. On augmenting the ordinates HP in some fixed ratio, we obtain a curve with deeper undulations, and augment the subnormals HN in the duplicate ratio; in this way we may cause the point N to pass beyond S before P has arrived at X. In such case N must become stationary, and then return to S, when P shall reach X; passing back towards R, N must again become stationary, and thereafter progressive, reaching T just when P does so. In this way the motion of N along RV will resemble the direct and retrograde movements in longitude of the superior planets. Parts of the line of abscissæ will thus be traversed thrice, once forward, once backward, and once forward again; and from each point of these parts three normals may be drawn to the curve. By properly adapting the ratio of enlargement we may cause the first stationary position of N to be at T, and then every part of the pitch-line is traversed thrice,—that is to say, wheels deduced from such a rack must always touch in three points. If we augment the ordinates in such a ratio as to bring the first stationary position of N onwards to U, every part of the pitch-line will be traversed five times, and the corresponding wheels will always touch at five places. In this way, when the general character or equation of the curve is determined on, we can discover the exact depth of tooth giving any specified odd number of contacts.

In machinery we should have at least two teeth completely engaged, for which there must be seven contacts, and the first stationary position of N must be brought forward to V, as is the case in fig. 8.

The most convenient process for tracing the shapes of wheel-teeth from such a rack is to determine the positions of the contact points corresponding to equidifferent positions of the wheels, and to combine the motion along the path with the proper angular motion of a blank disc. In this way we obtain very readily the outline of the wheel; this outline, however, though always giving the proper number of contacts geometrically, is not always mechanically possible; for low-numbered wheels it is exceedingly convoluted, as is seen in fig. 9, which is that of a wheel of *one* tooth, belonging to the system of fig. 8. As the number of teeth is augmented, the convolutions become less, and at a certain limit, the limit of mechanical possibility, they disappear.

Hence arises an exceedingly important and most difficult problem, "To discover that form of rack which, giving a determinate number of contacts, shall admit of the lowest numbered wheels." The idea of the curve of second sines occurred in the attempt to resolve this problem.

Here the condition of optimism cannot be put in the form of maximum or minimum, so that the known methods of analysis are inapplicable, and we must have recourse to successive trials with known or with invented lines, and after all we can only conclude that such or such a curve is preferable to any other that has been tried.

The simplest line, consisting of an endless series of equal and symmetric undulations, is the well-known curve of sines; this curve, when used as the form for a rack, gives convolutions on the outlines of wheels of considerable size, and it becomes desirable to obtain a better shape; for this we are naturally led to try modifications of the curve of sines.

If we write u for the absciss, y for the ordinate, and v for some variable arc, the equations

$$u = v + \phi \sin 2v ;\ y = \mathrm{A} \,.\, \theta \sin v ,$$

in which ϕ and θ represent two unknown functions, will give equidistant and symmetric undulations, it being essential, however, for our purpose that $\phi 0 = 0$; $\theta 0 = 0$; $\phi(-2) = -\varphi(+2)$ and $\theta(-2) = -\theta(+2)$. We have thus an endless variety of modifications among which to make our trials.

On omitting the term $\phi \sin 2v$, and putting $\theta \sin v = \sin \sin v$, we obtain the equation

$$y = \mathrm{A} \,.\, \sin {}^2 u \,,$$

that of the curve of second sines. The transition from the ordinary wave-line to this curve is abrupt, and symbolically of the same nature as the transition from the straight line to the curve of sines itself, as is seen on comparing the three equations

$$y = \mathrm{A} \,.\, u\,;\; y = \mathrm{A} \,.\, \sin u\,;\; y = \mathrm{A} \,.\, \sin \sin u\,;$$

but for the elucidation of the theory of wheel-teeth we require a gradual transition from the one kind of curve to the other; that is to say, we must obtain some comprehensive genesis which shall include both species of curve, and permit of an imperceptible change from the one to the other.

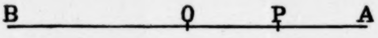

Fig. 1.

If a body vibrate along the straight line AOB, in virtue of some elastic arrangement whereof the redressing tendency is proportional to the distance from the mean point O, and if, while it is so vibrating, a sheet of paper be carried over it with a uniform velocity in a direction perpendicular to AOB, the trace made on that sheet is a curve of sines.

Instead of the rectilineal oscillation, let us use the motion of the balance-wheel of a watch—that is to say, let the vibration be in the circular arc AOB; and while the abscissæ, measured along the line RST, are made proportional to the times, let the ordinates be made equal to the sines *pp* of the arcs, instead of to the arcs O*p* themselves, and we shall have a variety of the curve of second sines.

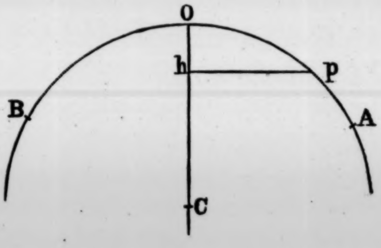

Fig. 2.

If the extent of the arc be small, its sine *pp* hardly differs from itself, and the curve merges into the ordinary curve of sines. When the length of the half-arc OA is just equal to the radius CO of the circle, we obtain the curve of second sines proper, which is represented in figure 1, the base RV being equal to the circumference of the circle, whose radius is CO. As the extent of the oscillation is augmented, the curve shows its tendency to flatten at the vertex X, and when the oscillation extends over the semicumference BAO, the curve, as shown in fig. 2, becomes quite flat at X, the radius of curvature there being infinite.

Where OA extends beyond the quadrant, the ordinate rises to be equal to the radius OC, and then decreases to reach a minimum value at X, after which it again rises, and so produces the saddle form seen in fig. 3; and when the oscillation extends over a complete turn, the vertex X of the curve comes down to S, as shown in fig. 4. If the oscillation extend over more than the whole circumference, the vertex X passes to the other side of the axis, as seen in fig. 5; and when the extent is one turn and a half, the curve is again flattened on the opposite limit, preparatory as it were, to the return towards S, when the oscillation is still farther extended. Thus this genesis produces a great variety of phases, beginning with the curve of sines, passing to the curve of second sines, and continuing in an endless series of variations beyond.

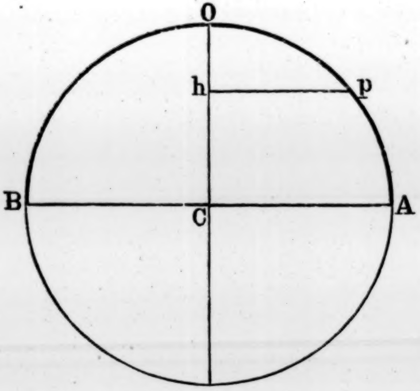

Fig. 3.

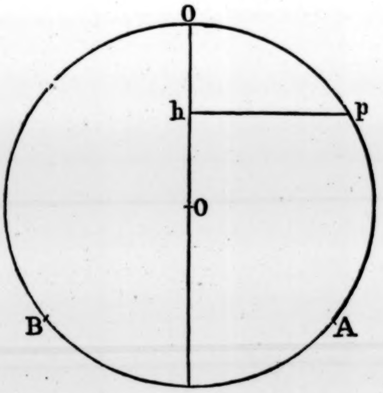

Fig. 4.

As soon as we pass beyond the flattened vertex, these curves lose all interest to the practical mechanician, who can hardly contemplate the use of wheel-teeth with hollowed tops; yet to the speculative engineer they offer the attraction of peculiar phases in the configuration of the relative contact-path, and in the convolutions of the tooth outlines; but their real interest is centred in this, that amongst them we find the best known form for the rack.

When the arc OA is three-fourth parts of a quadrant, and when the curve is raised to such a height as to have always seven normals from a point in the axis, wheels of 14 teeth, developed by its help, have their outlines mechanically complete.

Putting r for the length of OA, the half-arc of oscillation, the equation of the curve is

$$y = \mathrm{A} \,.\, \sin (r \sin u),$$

and the length of the HN subnormal is given by the formula

$$x = \frac{1}{2} \mathrm{A}^2 \,.\, \cos u \,.\, \sin (2r \sin u), \quad . \quad . \qquad (1),$$

which gives, at the same time, the form of the contact path. Hence we have RN, the length of the pitch-line, corresponding to the contact at P,

$$\mathrm{RN} = a = u + \frac{1}{2} r\mathrm{A}^2 \,.\, \cos u \,.\, \sin (2r \sin u), \quad . \qquad (2).$$

Hence if we denote by U that value of u which corresponds to the extreme position of the point N, we must have

$$\frac{2}{r\mathrm{A}^2} = \sin \mathrm{U} \,.\, \sin (2r \sin \mathrm{U}) - 2r \,.\, \cos \mathrm{U}^2 \,.\, \cos (2r \sin \mathrm{U}), \quad (3),$$

and when the number of contacts is to be n, we must have the corresponding value of RN equal to $\frac{n+1}{4}\pi$; wherefore

$$\frac{\tan (2r \sin \mathrm{U})}{\tan \mathrm{U} \,.\, \tan (2r \sin \mathrm{U}) - 2r \,.\, \cos \mathrm{U}} + \mathrm{U} = \frac{n+1}{2} \pi \, ; \qquad (4)$$

by help of which equation we can determine the values of U and A, corresponding to any assumed value of r, and to any desired number of contacts. For seven contacts, and when $r = \frac{3}{8}\pi$, we obtain $\mathrm{A} = 3{\cdot}469167$ and the maximum value of y, $3{\cdot}205089$; by help of which dimensions fig. 8 has been drawn.

If the point P be carried along the saddle-shaped curve RX of fig. 3, the subnormal HN lies first on the one and then on the other side of the ordinate PH, so that we may have two stationary positions of N as N_1 and N_2, and these may be placed so that the part $\mathrm{N}_2\,\mathrm{N}_1$ is traversed thrice, as actually happens in the figure. By lessening the ordinates the whole curve may be flattened, and the problem arises, "What must be the degree of flattening in order that the points N_1 and N_2 may coincide?" in which case no more than one normal can be drawn to the curve from any point in its axis.

Again, by augmenting the ordinates, as in figure 7, the extent of the overlap $N_2 N_1$ may be increased, and the limiting station for one quadrant may touch that for another quadrant of the curve, and thus we may determine the character and height needed to ensure that some specified odd number of normals, neither more nor less, may be drawn from any point assumed in the axis.

In the solution of such a problem we have to consider the two roots of equation 3 below $u = \frac{1}{2}\pi$, and recurring in each successive quadrant of the curve. When r exceeds π, and is less than $\frac{3}{2}\pi$, there are three such roots, as in fig. 5, and the discussion of the number of normals becomes exceedingly involved. The consideration of the wheel systems deduced from such curves belongs to purely speculative geometry.

3. Laboratory Note. By Professor Tait.

On the Thermo-electric Positions of Sodium and Potassium.

Farther experiments with the apparatus described in the "Proceedings" of 2d March 1874, and with a similar one containing potassium, have led to the following values of the tangent of the inclination of the corresponding lines in the thermo-electric diagram. I have added its value in palladium for comparison—

Na	−·00212
K	−·00066
Pd	−·00182

To reduce these to the corresponding numerical values of the specific heat of electricity, the factor required is 4×10^{-8} of a Grove's cell.

The line of Na in the diagram intersects that of Pb at about −20° C, and the line of K intersects that of Arg about the same temperature. By the help of these data they may easily be inserted in my diagram in vol. xxvii. part i. of the Transactions R. S. E.

4. On a New Form of Mariner's Compass. By Sir William Thomson.

Monday, 6th April 1874.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Further Note on Spectra under exceedingly Small Pressures. By Professor Tait and James Dewar, Esq.

2. On the After-glow of Cooling Iron at a Dull-Red Heat. By George Forbes, Esq.

The facts to be explained were observed by Messrs Gore and Barrett, and were described by the latter gentleman in the "Philosophical Magazine" for 1873.

The experiments are performed on an iron or steel wire of no great thickness. When this is heated to an intense white heat and allowed to cool, the following facts appear at the instant it has cooled down to a dull-red heat:—

1. The wire expands for an instant, and then continues its normal contraction.

2. The glow from the wire is at the same instant seen to increase.

3. The temperature of the air round the wire is at the same instant increased.

4. The same facts are seen when the wire is in an atmosphere of hydrogen.

5. If the wire be very thin the cooling is so rapid that the effects are not observed.

6. If the iron be massive the effects are not observed.

7. If the wire be not originally heated up to an intense white heat the effects are not observed.

That iron should increase its temperature at a dull-red heat while it is cooling from an intense white heat, and that it should not do so when cooling from a temperature a little over a dull-red heat, is a hypothesis so inconsistent with all known facts as to make it desirable to find some explanation more in accordance with known principles. Iron is a very bad conductor, and Professor Tait has shown (R. S. E. Proceedings, 1873) that the conductivity is much worse above than below a dull-red heat. Now, the cooling of such an iron wire as that used is effected so rapidly that the temperature falls through an enormous range of temperature in a few seconds. This is effected by convection and radiation from the surface. It is quite possible that the internal heat cannot be conducted outwards with sufficient rapidity to compensate this outer loss. Thus the temperature of the interior of the wire is greater than that of the exterior. At very high temperatures the rapidity of cooling is enormous. But as the cooling proceeds, the deviation from the Newtonian law of cooling is much less. Hence the cooling by radiation becomes less, and the heat which has been stored up in the interior of the wire has a tendency to show itself on the surface. At a dull-red heat the wire becomes a better conductor, and this tendency is assisted, so that about this stage the temperature throughout the wire is nearly equalised. The second experimental fact is explained by this raising of the external temperature. The third fact is explained in the same way. And it must be noticed, that a difference in temperature between the interior and exterior is the only means of explaining the rise in temperature of the external air, unless we suppose that, while cooling, the wire increases in temperature. And even then it would be difficult to understand why the effect is not produced by cooling from a temperature a little above a dull-red heat. If the wire be massive, or if a poker be used, the cooling is not rapid enough to produce the effects; apparently, because the convection currents are not nearly so strong in proportion to the surface which has to be cooled. Other causes come into play in this case, all tending to prevent the effect from being apparent. The explanation I have given shows why the effect is observed only when the wire has been originally heated to an intense white heat; for it is only then that a great difference of temperature can exist between the interior and exterior.

It only remains now to explain the first experimental fact, *i.e.*, the expansion of the wire at the critical instant. This follows from what has already been said, when we consider certain experiments made by Colonel Clarke, communicated to the Royal Society of London in 1863, and the explanation of them which was given by Professor Stokes. A hollow cylinder of iron was heated in a furnace, and plunged into water, so that half of it was buried in the water, the axis of the cylinder being vertical. After cooling, the cylinder was found to be permanently indented at the water-level, so that its diameter was there diminished. The explanation is as follows:—When plunged in water the lower part immediately contracts and cools. The upper part remains expanded. At this instant there is at the water-line a conflict between the upper, hot, expanded portion and the lower, cool, contracted portion. Now iron is much stronger when cool than when hot. Hence the cool iron has the advantage, and at the water-line the iron is at first forcibly shrunk, and afterwards cooled, and hence at that line the cylinder is contracted.

Now, exactly the same thing may happen in the cooling wire. Before cooling down to the dull-red heat, the hot inner part is expanded, and the cooler outer part contracted, and owing to the greater strength of the cooler iron, the wire is on the whole unduly contracted. But at the moment of after-glow the internal heat is driven out, and the contraction is no longer maintained. Hence the expansion at that temperature.

The hypothesis I have now given explains all the facts observed; but it cannot be stated to be proved. An alternative, and only one remains, which is to consider that *when iron is heated to an intense white heat it becomes different in its nature from cold iron, and that the iron in the hot state has a certain amount of latent heat, which is given out when, by cooling, the iron changes its nature.*

In the absence of any data for determining between these two, I prefer the former hypothesis, as it does not involve a new property of iron quite unlike that of any other substance yet examined. The apparently opposite phenomena observed when the iron is massive can be explained equally well on either hypothesis. But the second hypothesis is favoured by certain experiments made by Professor Barrett while heating the iron.

3. On a Form of Radiation Diagram.
By George Forbes, Esq.

The following facts appear to have been conclusively established by universal experience:—

1. Nearly all, if not all, solid substances become self-luminous at the same temperature.

2. The red rays are the first to become visible, and, on increasing the temperature, colours of less wave-lengths are successively added in the order of their wave-lengths.

3. While colours of shorter wave-lengths are being added, those which were previously visible become more intense.

It appears, then, that the intensity of radiation (i) of any particular colour is connected with the temperature θ, and the wave-length λ, by some equation

$$i = f(\theta, \lambda).$$

No data at present exist by means of which the form of this function can be determined. Theoretically, however, its determination is of great importance, and it also leads to some practical applications. For this reason it is worth while making an attempt to approximate roughly to a radiation diagram, on which shall be drawn curves that are isothermals, the ordinate of any point indicating the intensity of the radiation of a wave-length indicated by the abscissa, at the temperature of the particular isothermal considered.

The experimental data for an exceedingly rough approximation to such a curve exist. But difficulties of several kinds are met with.

1. If we judge the intensity of radiation by the eye, as Fraunhofer did, we can only see a limited portion of the spectrum; and if we use a thermo-pile, with the face covered with lamp-black, we have no proof that all the invisible rays are as thoroughly absorbed as we know the visible rays to be. A thermo-pile covered with chalk would not absorb the luminous rays so intensely as one coated with lamp-black. But we cannot say that lamp-black does not

behave like chalk, in this respect, to some of the invisible rays. In fact, Melloni's experiments show that this is the case.

2. The second difficulty is, that in employing the eye, a source of error is introduced by the fact that a certain intensity of radiation is necessary before a light becomes visible to the eye. This intensity (i') is evidently dependent upon the wave-length (λ) of the light considered. Hence

$$i' = \varphi(\lambda).$$

The difference between i and i', or

$$f(\theta, \lambda) - \varphi(\lambda)$$

gives a third curve,

$$\mathrm{I} = \psi(\theta, \lambda),$$

which shows the apparent intensity of any colour in terms of the temperature.

In this paper I wish to pay attention to the luminous portion of the spectrum, with the object of determining the nature of the function $\varphi(\lambda)$ as much as that of $f(\theta, \lambda)$.

I shall enumerate the different experimental facts which throw some light on the forms of these two curves.

1. Mossotti has shown* from the experiments of Frauenhofer that the curve of apparent intensities (*i.e.*, the curve $\psi(\theta, \lambda)$) is, in the case of sun-light, a sinuous line, symmetrical about the mean wave-length.

2. Draper† has shown that the radiation from the parts on eithe side of the mean wave-length are, with visible radiations equal.

Thus we are led to conclude that i and $i - i'$ are both symmetrical about the wave-length of mean visibility; and hence $i - i'$ or $\varphi(\lambda)$ is also symmetrical about that line.

3. Dewar‡ has shown that several methods combine to prove that the temperature of the sun is about 16,000° C.

Hence the isothermal on our diagram, corresponding to 16,000° C., has a maximum value of i for the mean visible wave-length.

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* Atti Scienz. Ital., 1843.

† Phil. Mag., 1872.

‡ Proceedings of the Royal Society of Edinburgh, 1871–72.

(Here we are only speaking of luminous radiations. But it is not improbable that in the scale of wave-lengths this is true in other parts. For refraction by prisms accumulates rays of different wave-lengths so much in the ultra-red part of the spectrum that no experiments exist which can settle this point.)

4. At lower temperatures the apparent maximum is nearer to the red, *i.e.*, the maximum of the curve $i - i'$ is nearer to the red.

But the curve i' is always a minimum at the yellow. Hence, at lower temperatures, the maximum of the curve $i = f(\theta, \lambda)$ passes to the region of greater wave-lengths. (It has just been stated that the curve $i = \varphi(\lambda)$ has a *minimum* at the centre of the diffraction spectrum. This is nearly certain, because we have seen that the curve is, at any rate, nearly symmetrical about this point, and it certainly increases enormously at the two limits of visibility of the spectrum.)

The only other remark I have to make on the curves $f(\theta, \lambda)$ is, that we cannot estimate the nature of the curve in the ultra-red at present. For all we know, there may be radiations of much greater wave-length than any which lamp-black, or any other substance we know of, could absorb.

As to the curve of limiting visibility, it appears, from what has already been said, that it has a minimum in the yellow; and although from Mossotti's interpretation of Frauenhofer's observations, it would seem to be a sinuous line, I do not think that the small variations there indicated could be detected accurately in judging of the relative brightness of different colours.

We can scarcely see those parts of the spectrum that lie beyond the lines A and H respectively. The question arises as to whether they are always invisible. If this were so, the curve of limiting visibility, which we have called $\varphi(\lambda)$ would, at those two points, be an ordinate of the curve.

But I do not think this is the case. So far as I can see, the limit of the spectrum depends upon the intensity of the light. Thus, Mr Glaisher, in his report to the British Association in 1863 on his balloon ascents, stated that, at great heights in the solar spectrum, he could "see H, and far beyond," when on the ground the line "H was quite the limit."

Again, I remember (although I cannot find a reference), that Sir

David Brewster increased the limits of the visible spectrum by dilating his pupil with Belladonna, so as to increase the amount of light.

From all these considerations, I believe that the diagram here given* is not a bad approximation to such a radiation diagram as I have described, data for its accurate determination being at present unobtainable. The principal Frauenhofer lines are marked below, and the numbers along the axis of abscissæ represent *thirteenth-metres.*†

I have drawn particular attention to what I have called the curve of limiting visibility, because a consideration of it affords an explanation of some curious facts which have, from time to time, been brought before the Royal Astronomical Society.

Many fellows of that Society were puzzled by the varying colours of stars, and of Jupiter particularly, when observed with different telescopes. Mr Huggins suggested that the amount of light, as depending upon the magnifying power and aperture of the object glass, might be the explanation of it. Mr Browning tested this, and found that it afforded a complete explanation. Colonel Strange corroborated these views by an independent observation.

I have tried, in a variety of ways, to produce this result experimentally, and believe that I have at length succeeded by employing gas light, and viewing it through a number of plates of the common blue glass coloured with cobalt. This thickness of the glass allows only blue and red rays to pass; the boundaries of these bright bands in the spectrum being sharply defined. When a piece of white paper, illuminated by the gas-flame, is examined with this glass, it appears to be blue, but the gas-flame itself appears to be red. This is due to no effect of fluorescence. Now, let I_r be the intensity of the red rays of the flame as seen through the glass, and I_b the intensity of the blue rays. Also let L_r be the limiting intensity for visibility in the red rays, and L_b in the blue rays. Then,

$$I_r - L_r \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (1)$$

and

$$I_b - L_b \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2)$$

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* The diagram here referred to is not reproduced. It will probably appear in a text-book on Physics now in preparation.—G. F., 1874, April 20.

† A thirteenth-metre = $10^{-13} \times 1$ metre.

are the apparent intensities of the red and blue parts. But, if $\frac{1}{n}$th only of the light from the flame is scattered from white paper; the intensities of the red and blue rays, when the paper is examined, are

$$\frac{1}{n}\,I_r - L_r \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

and

$$\frac{1}{n}\,I_b - L_b \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4).$$

Now, it is quite possible that while (1) is greater than (2), (4) should be greater than (3). Hence the light seen from the gas-flame has, on the whole, a red tinge, while that of the paper has a blue tinge, exactly as is seen to be the case.

I hope that this attempt at approximating to a better knowledge of some theoretically important facts will be of some interest to the Society, and that the meagre nature of our data will be sufficient apology for the small advance I have been able to make.

4. On the Semicircular Canals of the Internal Ear.

By Professor Crum Brown.

(*Abstract.*)

The author had laid before the Society, on the 19th January, a preliminary note containing an outline of a theory of the function of the semicircular canals of the internal ear. In that note it was stated that the six semicircular canals form three pairs—the members of each pair being parallel, and having their ampullæ at opposite ends. In this paper the author communicates the results of measurements of the position of the bony canals in a large number of animals.

The only manner in which, assuming bilateral symmetry, the canals can be arranged in parallel pairs, with the ampullæ at the opposite ends, is as follows:—In each ear, one canal at right angles to the mesial plane, and the two others making equal angles with the mesial plane. Calling the canals of the one ear a, b, c, and those of the other ear a', b', c'; a and a', b and c', c and b' are the three pairs; a and a' are coplanar, b is parallel to c' and c to b'. The measure-

ments show that this is approximately the case—deviations of 10° from parallelism being rare, even when, as is often the case, the three canals of one ear are not at right angles to one another.

The methods employed in making these measurements were explained and illustrated.

[*Note by the Author.*—Since presenting to the Society, on January 19th, the Preliminary Note on the Sense of Rotation and the Function of the Semicircular Canals of the Internal Ear, I have seen abstracts of papers on the same subject by Professor Mach and by Dr Breuer. As far as I can judge from these abstracts, while Professor Mach and Dr Breuer refer the action of rotation upon the ampullary nerves to the inertia of the contents of the canals, they do not seem to have noticed the parallelism of the plane of the superior canal of the one ear to that of the posterior canal of the other, nor to have observed that approximate parallelism of these planes is essential, if the semicircular canals are the peripheral organs of the sense of rotation.]

The following Gentlemen were admitted Fellows of the Society:—

R. H. TRAQUAIR, M.D., Mus. Science and Art.
FRANCIS JONES, Esq., Lecturer on Chemistry, Manchester.
W. F. BARRETT, F.C.S., R. College of Science, Dublin.

Monday, 20*th April* 1874.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On Last-Place Errors in Vlacq's Table of Logarithms.

By Edward Sang, Esq.

Now fifty years ago, while engaged with some heavy calculations connected with engineering work, I became impressed with the advantage of having logarithmic tables much more extensive than those in use. The trouble of the interpolations at the early part of the table, contrasted with the convenience of the small additional part from 100,000 to 108,000 printed in Hutton, gave rise to the idea of carrying the table onwards even so far as to one million. Although the bulk of such a table appears to be an objection, and the turning of so many leaves a toil, the ease to the habitual computer of finding at once the number of which he is in search is so great as far to outweigh the opposite considerations. Thus, though working only to five places, we prefer to use the extensive seven-place tables rather than to take up Lalande's small volume; and so, while working to seven places, we should gladly avail ourselves of a nine-place million table, the construction of which I proposed to myself, notwithstanding the vast amount of the labour.

The first idea was to interpolate from tables already published, but this was opposed by the feeling of dependency on the accuracy of the previous calculations. On examining the sources of our information on denary logarithms, it became apparent that the original work of Henry Briggs (1620), carried on in the laborious way indicated to him by John Nepair in his "Constructio," is the only foundation; and that the completion of the canon by Adrian Vlacq (1628) was the last of the original labour that has been bestowed on this matter so essential to the progress of exact knowledge.

The more convenient methods of calculation developed by the progress of logistics have come, as it were, too late to be of service. It is indeed surprising that, after the lapse of two hundred and fifty years, we are still relying on the unchecked calculations of Briggs and Vlacq; that among so many generations of scientific men there has not been zeal enough to effect a revision of the canon.

Even on the supposition that Vlacq's logarithms are true in the last place, the attempt to interpolate between them would lead to frequent uncertainty in the seventh place. In order to form an extensive table of seven-place logarithms true in the last figure, we should have to carry our original computations at least five steps farther.

Thus I came to perceive the necessity of making the whole computation anew. From time to time I took up the work to lay it down in alarm at its magnitude, for years of labour only seemed to make a beginning; but about 1849 I happened to obtain a copy of the great "Table des Diviseurs," by Burckhardt. The facility afforded by this admirable work for finding convenient formulæ of approximation, determined me to persevere in the construction of the large table; and, putting aside all my previous calculations, I arranged a comprehensive scheme for recording each step of the process, so that it might serve as occasion might arise to facilitate subsequent steps, and so that any suspected error might be traced to its source. By this means the progress of the work was effectually secured, because each little addition took its proper place, at however long an interval of time it might happen to be made.

Without going into the details of the procedure, it is enough to mention here that the logarithms of prime numbers up to 3600, and of many others occurring incidentally, have been computed to twenty-eight places with the view of being exact to twenty-five, and that the logarithms of all their products under 10,000 have been tabulated; and, by help of these, tables have been made to fifteen places of the logarithms of all numbers from 100,000 to 320,000, with their first and second differences. These, filling in all twenty-four quarto volumes, are laid on the Society's table.

Henry Briggs computed to fourteen places the logarithms of all numbers up to 20,000, and of numbers from 90,000 to 100,000; so that Vlacq, in shortening them to ten places, was safe from error excepting in one or two rare cases. But when Vlacq set himself to fill in the intermediate 70,000, he sought to lessen the labour by using only twelve places, thus making his last figure insecure in many more cases; and, moreover, the process followed by him wanted the quality of self-verification. On these accounts I suspected the occurrence of last-place errors in Vlacq's part of the table. Seeing that each tenth logarithm of my own computation from 200,000 to 300,000, should agree with Vlacq's from 20,000 to 30,000, the comparison was made, and the result was the discovery of forty-two errors in this single myriad—an exceedingly small number when the nature of the process is considered, but a very large number to have escaped detection for two centuries and a half. At the same rate for each of the remaining six myriads, we may expect a total of nearly three hundred errors.

In 1658, that is thirty years after Vlacq, John Newton published a translation of Gellibrand's "Trigonometria Britannica," in which he gives an eight-place table of logarithms arranged in the compact manner now usually adopted. In the address to the reader, he speaks contemptuously of Adrian as "Vlaq the Dutchman," "from whose corrupt and imperfect copy," &c.; and in the introduction he describes a mode of computing logarithms which the innocent reader may believe to have been followed by the author of the book, but a collation shows that Vlacq's misprints have been slavishly copied by the indignant Newton.

It was not until 1794 that anything claiming to be a revision of the original table appeared; this was the ten-place table given by Georg Vega in his "Thesaurus Logarithmorum," the arrangement being after the compact manner introduced by Newton. Vega gives a long list of corrections on Vlacq's table, which by that time had become scarce, and it was generally understood that he had at least taken the precaution of adding up Vlacq's differences in order to eliminate the misprints. But on collating the list of errors which I have just discovered in Vlacq, with Vega's table, we are forced, however reluctantly, to the conclusion that Vlacq's identical table had been used by the compositor of Vega's pages. A review of the character of the errors will make this clear; a list of them is subjoined, showing the logarithms true to fifteen places (the first five being omitted), the last group as it should have been in Vlacq, Vlacq's corresponding five, and Vega's last group of three.

Of the forty-two errors shown in Vlacq, forty are last-place errors, such as we are considering; and two, marked with asterisks, are misprints, as is known by the circumstance that the adjoining differences are correct. As was to have been expected, all the final errors are copied by Vega, who never pretended to have made a new computation; of the misprints one, a 9 for a 6, is corrected; but the other, 646 instead of 626, is retained. Not only so, among the final errors there are six belonging to numbers ending in 0; now these logarithms occur in the preceding part of the table, where they are correctly given, and yet these also, of easy detection, are retained by Vega. Thus, again, Vega is only Vlacq in a new and much more convenient form.

The only work claiming to be an original computation of logarithms is that done in the Bureau du Cadastre, at the instance of the French Government. This unpublished work contains to nineteen places the logarithms of numbers from 1 to 10,000, and to fourteen places of those from 10,000 to 200,000. In the year 1819 the House of Commons, on the motion of Mr Davies Gilbert, presented an address to the Prince Regent, recommending that our Government should join with that of France in the expense of publishing these and the accompanying Trigonometrical Tables; but the negotiations fell through, for reasons that have not been made public. I have not learned that these computations have been used for the verification of those already printed, or that they have served for the production of any seven-place table; and thus, up to the present moment, we have no verification of Vlacq's great work.

The eminent astronomer Lalande, in publishing his little five-place table, was able confidently to assert that it does not contain a single error, and although many thousands of copies have been in use now for seventy years no fault has been detected. Thus the production of a faultless table is quite within the range of possibility; it is a matter of time, of care, of expense; and with our modern appliances the endless reproduction of the plates is easy; so that computers ought to be in possession of tables trustworthy throughout, especially of such tables as are of universal application.

Though not needed for the every-day work of the computer, tables of excessive precision are not the less needed in special departments, and in the preparation of other tables for ordinary use. Their extent and the expense of preparing them, coupled with the smallness of the number of those by whom they are desired, precludes their preparation by private parties, and relegates the matter to the care of public authorities.

In the same way that the "Nautical Almanac," which is far beyond the reach of private enterprise, and yet is needed for the advancement of navigation and astronomy, is undertaken by the Government, it would be right to carry out the idea of Davies Gilbert, and to confer, by the publication of exact tables, a similar boon upon the other branches of science.

It would be fitting that this should be done by the British Government, seeing that the invention and completion of the logarithmic method belong to the Island; and it would be not less fitting that the first public body to move in the matter should be the Royal Society of Edinburgh, from whose place of meeting we could almost have seen the roof under which John Nepair elaborated his invention, and could fancy to have heard the creaking of the screw with which Andrew Hart imprinted the "Canon Mirificus."

Number.	Log. to 15 Places.	To 10.	Vlacq.	Vega.
20071	90109 36054	90109	90110	110
20280	79506 61298	79507	79506	506
20375	76174 12014	76174	76175	175
20645	48872 10721	48872	48873	873
20822	24421 41256	24421	24422	422
20866	92030 46050	92030	92031	031
21245	67354 25533	67354	67355	355
21749	92932 69231	92933	92932	932
21795	68733 53703	68734	68735	735
21904	34307 89915	34308	34309	309
22016	84165 55417	84166	84167	167
22200	29744 50639	29745	29744	744
22312	85012 57993	85013	85012	012
22877	90721 80887	90722	90721	721
22996	22999 73937	23000	22999	999
23274	10299 83700	10300	10299	299
23492	99921 70919	99922	99923	923
23820	17571 46759	17571	17572	572
24156	50209 36279	50209	50210	210
24580	18785 50435	18786	18785	785
25173	49758 10852	49758	49759	759
25524	87359 50354	87360	87359	359
25586	23955 50655	23956	23955	955
25707	13975 50452	13976	13975	975
26004	01573 67443	01574	01573	573
26407	90654 45820	90654	90655	655
26517	43886 18717	43886	43889*	886
26642	68239 65258	68240	68239	239
26699	49953 49034	49953	49954	954
26717	76904 57995	76905	76904	904
26728	64626 30075	64626	64646*	646
27291	94494 30434	94494	94495	495
27560	92132 35588	92132	92133	133
27586	87318 72159	87319	87318	318
27861	67002 67696	67003	67002	002
27921	09686 32521	09686	09687	687
28486	14699 52392	14700	14699	699
28680	91469 95763	91470	91469	469
29226	93799 55414	93800	93799	799
29446	63077 50861	63078	63077	077
29639	35467 49658	35467	35468	468
29703	03152 31285	03152	03153	153

2. Note on the Submerged Fossil Trees of Granton Quarry.

By Sir R. Christison, Bart., Hon. V.P., R.S.E.

It may interest those who are acquainted with the history, structure, and composition of the Craigleith fossil trees, described in the two papers recently read to this Society, to learn that an opportunity has occurred for examining comparatively specimens from the submerged fossil trees of Granton Quarry. The specimens were preserved by Mr Hawkins, engineer of the Granton harbour, and have been, through his kindness, not only subjected to examination, but also presented for preservation to the Botanic Garden collection.

It turns out that the microscopic structure and chemical composition of the greater of the two Granton fossils are precisely the same with the structure and composition of the fossils of Craigleith, two miles distant. The embedding rock is also the same in composition, whether in its pure state, or where altered by percolating water. That is, the microscopic structure of the Granton fossils appears to be that of the pine tribe; and the fossilising material consists of the carbonates of lime, magnesia, and iron, all in notable proportion; and while the fundamental rock of the quarry is a very pure quartzy sandstone, without any binding calcareous carbonate, many masses may be seen among the blocks raised many years ago from the quarry, but not made use of, which like similar altered specimens from Craigleith, have their fracture, colour, and toughness changed by the same material which has fossilised the trees. In the fossils, too, there is the same three or four per cent. of charcoal left after the solvent action of acids on the fossils of Granton as on those of Craigleith.

That part of Mr Witham's fossil of 1830 which lay in front of the Museum of Science and Art, has now been removed to the Botanic Garden, to be added to the lower part of the trunk of which it is the continuation. In separating two of the segments, a cavity was found which contained a matter like charcoal, some fragments of which even presented the fibrous appearance of charcoal to the naked eye; and Mr Sadler, of the Botanic Garden, has ascertained that some of these fragments show before the microscope the characteristic punctated structure of the vertical section of the pine family. This, I believe, is the first time that this particular part of the pinaceous structure has been observed in any of these fossils.

It may be farther noticed that there are now in the Botanic Garden Museum two great polished slabs, nearly three feet in diameter, from the Craigleith fossil last discovered,—one of which shows in many places to the naked eye the annual layers of wood concentrically; and that in breaking up a large mass of the same fossil, in the hope of discovering a deposit of charcoal in a cavity, several fine fractures were obtained, showing distinctly to the naked eye large surfaces of the ribbon-like structure of the transverse medullary rays, and one surface presenting to the naked eye not only these markings, but likewise the annual layers cut vertically.

3. Note on Grouse Disease. By Professor Maclagan.

The result of the author's examination of diseased birds has been to confirm the statements of Drs Cobbold and Crisp, lately published in the "British Medical Journal," that diseased grouse, or at least the emaciated birds commonly known as "piners," owe their depraved condition to a small thread-like worm (*Strongylus pergracilis*, Cobbold) which infests the cæca. The author concurs in the opinion entertained by most of those who have written on the subject, that the tape-worm (*Tænia calva*), which is well known to infest the grouse, is not the cause of the disease. The *Tænia* is undoubtedly often present along with the *Strongylus* in diseased birds, but is often found by itself in plump healthy grouse. The worst cases seem to be those in which both are present in quantity, as in one examined by the author, whose cæca was crowded with *Strongyli*, whilst the intestine contained ten tape-worms, the whole weight of the bird, a full-grown cock, being only 15½ ounces. It is not possible accurately to determine the number of *Strongyli* in any one case, but, so far as it could be determined, it appeared that the more numerous were the *Strongyli* in any one bird, the greater was its emaciation. By a rough but moderate calculation the author was led to estimate the number of these worms in one of his birds as at least 4800.

There is, however, no definite line between birds with and those without this disease, for almost every grouse is the "host" of fewer or more of the parasites. Of eleven birds carefully examined by the author, with the aid of Mr Stirling of the University Anatomical Museum, in one only, a fine Irish cock weighing one pound eleven ounces, were none detected. The other birds examined were from various localities: those containing the fewest worms, and of the heaviest weight, were from Ireland and Orkney; those from Lanarkshire, East Perthshire, and Sutherland were the most affected by the worm and most emaciated.

So far the author concurs with the writers named above, that one form of grouse disease is this helminthiasis, due to the *Strongylus*, which destroys the birds by ultimately annulling the functions of the cæca, in which the real digestion of the birds' food goes on. The mucous membrane is not inflamed, but irritated, throwing off great quantities of large columnar epithelium, and instead of true fæculent matter, or remains of food, the intestines and cæca usually contain only a pinkish grey mucus. The cæca, however, seem occasionally to be softer and more easily torn than is natural. In none of these birds was any other morbid appearance found capable of accounting for their morbid state.

It is not yet clear to the author, however, that this helminthiasis is *the* disease which has so often swept the moors of Scotland and England. It is quite possible it may be so, and there is nothing in its rapid spread on particular moors in certain seasons to prevent its being due to parasites; but the author thinks that further inquiry is desirable, and, speaking as a sportsman, would suggest to the proprietors and tenants of moors, which are now so valuable as to be a subject of national importance, to raise by subscription a sufficient fund to enable them to commission some competent naturalist to work out the subject. The genesis of the worms, both *Strongyli* and *Tæniæ*, in a scientific point of view, irrespective of the hope of some practical conclusion, appears to be worth the expenditure of some money.

4. Latent Heat of Mercury Vapour.
By James Dewar, Esq.

5. Notes by James Dewar, Esq. (1.) Problems of Dissociation; (2.) Formation of Allotropic Sulphur; (3.) Heat of Fermentation.

6. Further Note on Continuants. By Thomas Muir, M.A., F.R.S.E., Assistant to the Professor of Mathematics in Glasgow University.

In my paper on Continuants, recently communicated to the Royal Society, it was shown that the order of a continuant may be depressed if the first element of the main diagonal be unity, viz., thus:—

$$\mathrm{K}\left(\begin{array}{ccccccc} & b_1 & & b_2 & & b_3 & \\ 1 & & a_1 & & a_2 & & a_3 \ldots \end{array}\right) = \mathrm{K}\left(\begin{array}{ccccc} & b_2 & & b_3 & \\ a_1+b_1 & & a_2 & & a_3 \ldots \end{array}\right)$$

and from the definition it is evident that

$$\mathrm{K}\left(\begin{array}{ccccc} & mb_1 & & b_2 & \\ ma_1 & & a_2 & & a_3 \ldots \end{array}\right) = m\,\mathrm{K}\left(\begin{array}{ccccc} & b_1 & & b_2 & \\ a_1 & & a_2 & & a_3 \ldots \end{array}\right)$$

Hence we have

$$\mathrm{K}\left(\begin{array}{ccccccc} & b_1 & & (b_1+a_1)b_2 & & (b_2+a_2)b_3 & \\ 1 & & a_1 & & a_2 & & a_3 \ldots\ldots \end{array}\right)$$

$$= \mathrm{K}\left(\begin{array}{ccccc} & (b_1+a_1)b_2 & & (b_2+a_2)b_3 & \\ b_1+a_1 & & a_2 & & a_3 \ldots\ldots \end{array}\right)$$

$$= (b_1+a_1)\mathrm{K}\left(\begin{array}{ccccc} & b_2 & & (b_2+a_2)b_3 & \\ 1 & & a_2 & & a_3 \ldots\ldots \end{array}\right)$$

$$= (b_1+a_1)\,(b_2+a_2)\,(b_3+a_3)\ldots\ldots$$

From this it is clear that, in virtue of the relation which has given rise to the name "continuant," continued fractions of a certain class may be transformed into simple fractions, with continued products for numerator and denominator.

A general theorem on this transformation is given by Stern in "Crelle's Journal," vol. x. (1833), p. 267 ; and by a quite similar method several allied identities have been reproduced in a paper recently read before the Mathematical Society of London. All of them, however, may be established much more easily by means of the above results. Thus—

$$1+\cfrac{d_1-e_1}{e_1-\cfrac{(d_2-e_2)\,d_1e_1}{d_1d_2-e_1e_2-\cfrac{(d_1-e_1)\,(d_3-e_3)\,d_2\,e_2}{d_2d_3-e_2e_3-\cfrac{(d_2-e_2)\,(d_4-e_4)\,d_3e_3}{d_3d_4-e_3e_4-\cdots\cdots}}}}$$

$$=\frac{K\left(\begin{matrix} & d_1-e_1 & & -(d_2-e_2)d_1e_1 & & -(d_1-e_1)(d_3-e_3)\,d_2e_2 & \\ 1 & & e_1 & & d_1d_2-e_1e_2 & & d_2d_3-e_2e_3\,\ldots\ldots \end{matrix}\right)}{K\left(\begin{matrix} & -(d_2-e_2)\,d_1e_1 & & -(d_1-e_1)(d_3-e_3)\,d_2e_2 & \\ e_1 & & d_1d_2-e_1e_2 & & d_2e_3-e_2e_3\,\ldots\ldots \end{matrix}\right)}$$

$$=\frac{d_1\,.\,d_2\,(d_1-e_1)\,.\,d_3\,(d_2-e_2)\,\ldots\ldots}{e_1\,.\,e_2\,(d_1-e_1)\,.\,e_3\,(d_2-e_2)\,\ldots\ldots}$$

$$=\frac{d_1d_2d_3\,\ldots\ldots}{e_1e_2e_3\,\ldots\ldots}$$

which is the general result obtained by Stern, and which probably includes all the others.

I am indebted to Professor Cayley for the remark that any continuant may be expressed by means of a simple continuant. Thus dividing the second column by the first constituent of it which is not zero, and then multiplying the third row by the same, and so on through the remaining columns and rows in succession, we have

$$\begin{vmatrix} a_1 & b_1 & 0 & 0 & 0 \\ -1 & a_2 & b_2 & 0 & 0 \\ 0 & -1 & a_3 & b_3 & 0 \\ 0 & 0 & -1 & a_4 & b_4 \\ 0 & 0 & 0 & -1 & a_5 \end{vmatrix} = \begin{vmatrix} a_1 & 1 & 0 & 0 & 0 \\ -1 & a_2\frac{1}{b_1} & 1 & 0 & 0 \\ 0 & -1 & a_3\frac{b_1}{b_2} & 1 & 0 \\ 0 & 0 & -1 & a_4\frac{b_2}{b_1b_3} & 1 \\ 0 & 0 & 0 & -1 & a_5\frac{b_1b_3}{b_2b_4} \end{vmatrix} \times b_2b_4$$

And so generally

$$\mathrm{K}\left(\begin{array}{ccccccc} & b_1 & & b_2 & & b_{n-1} & \\ a_1 & & a_2 & & a_3 \ldots & & . \, a_n \end{array}\right)$$

$$= \mathrm{K}\left(a_1 \, , a_2 \frac{1}{b_1} \, , a_3 \frac{b_1}{b_2} \, , a_4 \frac{b_2}{b_1 b_3} \, , a_5 \frac{b_1 b_3}{b_2 b_4} \ldots \ldots , a_n \frac{\ldots . b_{n-4} b_{n-2}}{\ldots . b_{n-3} b_{n-1}}\right) \times \ldots b_{n-3} b_{n-1} .$$

To this may be added the following as being derived in similar fashion :—

$$\mathrm{K}\left(a_1 x^{-1}, a_2 x \, , a_3 x^{-1}, a_4 x \ldots . a_n x^{(-1)^n}\right) = \mathrm{K}\,(a_1 \, , a_2 \, , a_3 \ldots a_n)$$

if n be even, and

$$= \mathrm{K}\,(a_1 \, , a_2 \, , a_3 \ldots a_n) x^{-1}$$

if n be odd.

The simple continuant I have found to be identical with Euler's "*Novus Algorithmus.*" An examination of his paper with this title will at once make evident the advantages of the new mode of considering the function.

Monday, 4th May 1874.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read :—

1. On the Formation of Allotropic Sulphur.

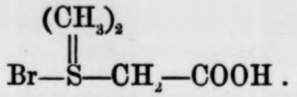
By James Dewar, Esq.

2. On Some Compounds of Dimethyl-Thetine. By Professor Crum Brown and Dr E. A. Letts.

(*Abstract.*)

In this paper the authors describe in detail compounds of dimethyl-thetine, some of which were enumerated in an earlier communication. Hydrobromate of dimethyl-thetine is readily obtained as a colourless crystalline body by the action of sulphide of methyl on bromacetic acid at ordinary temperatures. Its analysis shows that it contains quantities of the different elements agree-

ing with the formula $C_4H_9BrSO_2$, and its mode of formation and reaction lead to the constitutional formula —



$$Br—\overset{(CH_3)_2}{\overset{\|}{S}}—CH_2—COOH\,.$$

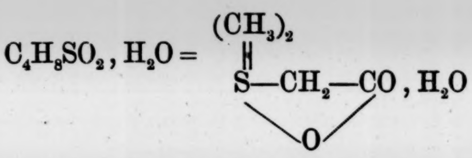
The result of its analysis are as follows :—

Calculated.			Obtained.	
C_4 =	48	23·9	23·7	23·1
H_9 =	9	4·5	4·5	4·8
O_2 =	32	15·9	—	—
S	32	15·9	—	16·1
B_1	80	39·8	40·0	39·7
	201	100·0		

It is a very deliquescent body, and has a powerful acid reaction.

Dimethyl-thetine.—The base, of which the body just described is the hydrobromate, is obtained from the latter by the action of oxide of silver. It may also be prepared from the sulphate of dimethyl-thetine by treatment with carbonate of baryta. It is a very deliquescent crystalline body, containing one molecule of water of crystallisation, which it loses if exposed for several days over sulphuric acid *in vacuo*.

The numbers calculated for the formula



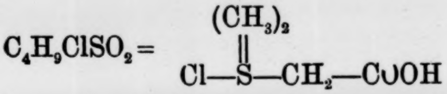
$$C_4H_8SO_2\,,H_2O = \overset{(CH_3)_2}{\overset{\|}{S}}—CH_2—CO\,,H_2O$$

agree with those obtained by experiment, thus—

Calculated in 100.		Obtained.	
C	34·7	34·0	33·9
H	7·2	7·3	7·3
H_2O	13·0	18·0	—

Hydrochlorate of dimethyl-thetine.—This body may be obtained either by saturating a solution of the base with hydrochloric acid, or by means of double decomposition between the sulphate of dimethyl-thetine and chloride of barium. It is a crystalline substance of strong acid reaction.

The formula



$$C_4H_9ClSO_2 = \overset{(CH_3)_2}{\overset{\|}{Cl{-}S{-}CH_2{-}COOH}}$$

was verified by a chlorine determination.

Calculated in 100.	Obtained.
Cl ...... 22·7	23·4

Chloroplatinate of dimethyl-thetine is obtained in beautiful light orange-coloured crystals when solutions of the hydrochlorate and chloride of platinum are mixed. This salt contains two molecules of water of crystallisation, and has the formula

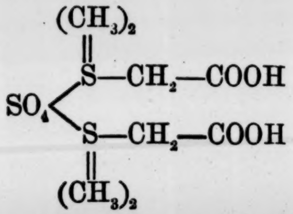
$$2(C_4H_9ClSO_2), PtCl_4, 2H_2O.$$

The salt was analysed by determination of water and platinum—

Calculated in 100.		Obtained in 100.
Pt	= 28·6	28·5 ...... 28·4
H_2O	= 5·2	5·4 ...... —

Bromaurate of dimethyl-thetine was obtained by mixing alcoholic solutions of hydrobromate of methyl-thetine and bromide of gold. The analysis shows too small a quantity of gold for the normal bromaurate, but agrees with the amount required for a salt crystallising with three molecules of alcohol. This compound has not been more particularly examined.

Sulphate of dimethyl-thetine was prepared by the action of sulphate of silver on hydrobromate of dimethyl-thetine. It can be obtained in large crystals, which are not deliquescent. It was analysed by a sulphuric acid determination.

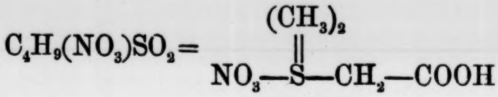


requires 28·4 per cent. SO_4

whereas 28·2 per cent. and 28·0 per cent. were obtained by experiment.

Nitrate of dimethyl-thetine, prepared by treating the hydrobromate with nitrate of silver, is a transparent crystalline substance.

The formula



$$C_4H_9(NO_3)SO_2 = NO_3-\overset{(CH_3)_2}{\overset{\|}{S}}-CH_2-COOH$$

was verified by an organic analysis—

	Calculated.	Obtained.
C	26·2	26·4
H	4·9	4·9

Double Salt of dimethyl-thetine and bromide of lead.—A boiling solution of hydrobromate of dimethyl-thetine dissolves carbonate of lead with evolution of carbonic acid, and on cooling deposits beautiful silvery scales.

The formula of this body is

$$C_4H_8SO_2\,,\ 2PbBr_2$$

	Calculated.	Obtained.	
C	5·6	5·7	5·7
H	·9	·9	·9
Br	37·0	37·0	37·5
Pb	48·4	48·4	48·5

In addition to these compounds, a very beautiful salt was obtained by the action of hydriodic acid on the base, or by double decomposition between iodide of barium and sulphate of dimethyl-thetine. In appearance it resembles permanganate of potash, and is a poly-iodide; but its examination is not as yet completed.

Hydrobromate of diethyl-thetine was prepared in a similar manner to the corresponding dimethyl compound, which it resembles; it is so deliquescent, however, as to render its analysis almost impossible. It gives beautiful orange-coloured salts with bichloride of platinum.

In the course of these experiments the action of iodacetic ether upon sulphide of methyl was studied. The reaction here takes a different course, iodide of trimethyl-sulphine being produced in large quantity.

3. On a New Example of the Opheliidæ (*Linotrypane apogon*)* from Shetland. By W. C. M'Intosh.

This peculiar iridescent pinkish Annelid was dredged in 1871 in Bressay Sound, in four or five fathoms, on a bottom of coarse sand and gravel, which abounded with finely-branched *Melobesia calcarea*, Ell. and Soland.

The form resembled an active nematoid worm, being elongated, nearly cylindrical throughout the greater part of its length, and devoid of bristles or lateral projections. It progressed in the most vigorous and spasmodic manner, by twisting or thrusting itself through the sand, after the mode of *Ammotrypane*, or a most rapid eel-like fish. Moreover, the slightest interference caused it to break in pieces, so that not a single specimen out of the whole series remains entire, though every precaution was taken to immerse the animals in spirit on removal from the dredge. The activity and purpose displayed by the species are diagnostic when compared even with the most nimble of the nematoid group, so that no difficulty is experienced in distinguishing it.

The Annelid reaches the length of three or four inches, and is only about a millimetre ($\frac{1}{25}$th inch) in diameter. The body is rounded, slightly tapered in front, where the pinkish colour is best marked, and richly iridescent, even to a greater degree than either *Ammotrypane* or *Ophelia*. The head terminates in a rounded anterior border, from which two short clavate processes project. The latter have a very thin investment of the hyaline cuticle, with a thick layer of granular cells (hypoderm) beneath. Some longitudinal fibres occur at the base, but the contractility of the organs is limited. Two eyes, consisting of encapsulated masses of black pigment, are situated near the dorsal surface of the tissues of the snout.

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* λινον, a thread, and τρυπάνη; the specific name from ἀπωγων, beardless

Body-wall.—The external investment is a translucent, perfectly smooth, glistening cuticle, very thin on the snout, cephalic processes and the anterior region, but of considerable thickness and great tenacity throughout the rest of the body. It is this layer which enables such forms to bear much strain in a longitudinal direction, and, by its great elasticity, to dispense with a special circular layer of muscular fibres. In some of the Nemerteans, for instance, where the cutaneous tissues are soft and easily injured, a very perfect circular muscular coat occurs next the basement-membrane of the latter, and exterior to the longitudinal layer. When a single layer of this hyaline cuticle is examined, after mounting in chloride of calcium, a number of puncta, arranged with greater or less regularity, and apparently passing quite through it, are found. By tearing with needles, or examination in simple water, it is further seen to be composed of a closely interwoven series of very fine fibres, many of which have a crossed-spiral, or oblique direction. This is a common arrangement in such iridescent forms. The cuticle readily separates from the subjacent layers in the preparations, a feature less evident in *Ammotrypane* and *Ophelia.* Beneath the foregoing is a cellulo-granular layer (hypoderm), which in transverse sections preserves a nearly uniform thickness, except inferiorly, where the nerve-cords occur. The cells vary in size, are filled with granules, and embedded in a hyaline intercellular substance. Many granules also exist amongst the cells. In the cephalic region a considerable thickening of the coat takes place, especially inferiorly, and this enlargement coincides with the diminution of the hyaline cuticular layer formerly mentioned. A boundary or basement-layer occurs on the inner surface.

Within is a great longitudinal muscular coat, which (besides the passage of the oblique muscular fibres) is interrupted at two points in its circumference, viz., at the median line of the dorsum, and the opposite point inferiorly. The former is but a faint separation, caused by the suspensory fibres of the alimentary region; the latter is a boldly-marked hiatus—the inferior fibres of the alimentary canal, the oblique muscular bands of the body-wall, and the ventral blood-vessel meeting at this point. In ordinary transverse sections this coat presents a somewhat wavy, radiated appearance, from the arrangement of the fasciculi. In stating that the direction of the muscular fibres in such sections is radiated, some explanation is perhaps necessary, for, while the fasciculi of the dorsal and lateral regions point more or less in this way, the arrangement at the raphe is different, since the oblique bands, passing down at an acute angle, direct, in the contracted state, the fasciculi upwards and outwards. They gradually become vertical, and then slant in the opposite direction, before leaving what may be termed the ventral region. A firm band, apparently of the limiting membrane of the hypoderm, proceeds from angle to angle at the raphe.

From this coat, at somewhat regular intervals, pass a series of muscular bridles, each forming a kind of diaphragm (dissepiment). Most of the fibres have a vertical direction. The same arrangement is observed in the Nemerteans and in most of the Annelida. Such bundles, of course, are altogether independent of the characteristic oblique bands of muscular fibres which pass from the lateral dorsal region on each side to the raphe at the ventral edge. Anteriorly the latter bands form, in contraction, a curve on each side, with the convexity directed inwards, and they enclose a somewhat elliptical portion of the great longitudinal layer, with a few cells and granules. The oblique bands spring from the basement-membrane, and thus pass through the longitudinal layer,—an arrangement very well seen in front, where the bands are of great thickness. Posteriorly the comparative slenderness of the oblique muscles makes this subdivision of the longitudinal layer indistinct, but it is nevertheless present. In this region, also, the distance between the middle of the oblique band and the longitudinal coat is considerable, the space being filled with cellular tissue and a few fibres.

It will thus be observed that the animal has a very complete muscular system, relatively of great power, for the execution of its remarkable boring propensities in sand and gravel.

Digestive System.—The mouth opens in the preparations on the ventral surface, a short distance behind the tip of the snout, and has prominent lips. It leads into a richly ciliated digestive chamber, which runs to the posterior end of the body. No dental organs of any kind exist, the food apparently consisting of sand or sandy mud, requiring nothing more than simple engulfment. Anteriorly, what may be termed the œsophageal division of the canal has internally a well-defined margin, covered with closely-set cilia, the wall consisting of the usual granular gland-cells, embedded in a hyaline stroma, with muscular fibres. Posteriorly, it is more opaque and granular, and appears to end in an anus without processes. All the specimens, however, were imperfect. The organ is thrown into innumerable rugæ internally; while externally it is kept in position by the dorsal and ventral fibres formerly noted, as well as by the dissepiments. The broad inferior fibres pass to the transverse band at the raphe, and a few even extend in some sections to the exterior border of the cellular coat in this region, at the nerve-cords.

Nervous System.—It is somewhat difficult to make out the arrangement of the cephalic ganglia in the specimens; but they are situated in the snout, near the eyes, and form two slightly tinted masses, terminating on each side in a buccal cord, which passes downwards to the ventral surface, and extends along the body beneath the transverse band of the raphe. The cords are larger in front, and somewhat farther apart, but throughout the rest of the body are closely approximated. The usual granular sheath surrounds them, and they are also protected by part of the cellular coat inferiorly.

In comparing the foregoing form with the representatives of the *Opheliidæ* at present described, it is at once distinguished by the absence of bristles. In *Ammotrypane* the united nerve-cords are situated at the ventral edge of the T-shaped prolongation of the body-wall inferiorly, and have a muscular column between them and the perivisceral cavity. In *Ophelia* the nerve-cord lies within the great longitudinal muscular cord, at the junction of the ventral prolongations (in transverse sections). The body-wall differs in the relative thickness of the several layers, and especially in the great bulk of the cellular coat in the new form. One of its nearest allies seems to be a new *Ammotrypane* dredged in Valentia harbour by Dr Gwyn Jeffreys, which shows a very minute trace of bristles, though the form of the body closely agrees with the *Ammotrypane aulogaster* of H. Rathke. In the Irish species, however, the united nerve-cords lie between the ventral ends of the powerfullydeveloped oblique muscular bands which separate the longitudinal coat in the median line inferiorly.

The occurrence of an Annelid proper devoid of bristles is an interesting fact; for, though such organs are feebly developed in *Tomopteris*, they have been considered on the whole so universal, that, for example, the two great divisions Polychæta and Oligochæta rest thereon. The new form likewise shows no trace of segmentation externally, in this respect agreeing with the Nemerteans, yet in structure it is truly an Annelid proper. It is difficult to assign its exact position at present, and the association with the Opheliidæ may be regarded as provisional.

4. The following concluding Remarks were made by Mr D. Milne Home, who occupied the Chair in room of the President:—

I. I have been requested by our Secretary to announce formally from the Council, that this is the last meeting for the Winter Session.

You will have seen from the billet, that our President, Sir William Thomson, was to have been in the chair to-night, and to have closed the session with some remarks suitable to the occasion.

The Council are much disappointed, and no doubt you also are; but I am more distressed than any one at Sir William Thomson's absence. There is a letter from him to the Secretary, dated on Friday last, mentioning that he could not attend this evening, as he expected to be in his yacht to-day in the Bay of Biscay.

The Council, therefore, had no alternative but to appoint me, as the only Vice-President at hand, to occupy the chair to-night. The occupation of the chair is unaccompanied by any difficulty,—but the other duty, of offering concluding remarks worthy of your acceptance, I find it simply impossible to perform. I am sure you will neither expect it, nor wish me to attempt it.

Such remarks, therefore, as I shall offer, will be matter of mere form, and will not contain thoughts or suggestions, or information of any scientific value.

II. I must, however, detain you for a few moments in adverting to our proceedings during the past winter.

1. I think from my recollection of the papers which have come before our meetings, that we have attended to most of the objects for which our Society was established.

We have had papers on the various physical sciences—Chemistry, Natural Philosophy, Geology, Botany, Mathematics, Anatomy, and Zoology. But besides science, our Society was intended for the encouragement of Literature; and I regret to say, that I do not remember any paper read this winter of a literary character.

At our last meeting, one of the papers was by Mr Sang on Logarithms, and he produced on our table no less than 20 MS. volumes of logarithms to 15 places of decimals! the publication of which, he pointed out, would be of great advantage to astronomers and others who require the aid of logarithms in their calculations, and accompnaied by minute accuracy. That opinion was publicly confirmed by other members of the Society very competent to judge; and I may now announce, that our Council have, in accordance with that opinion, come to the resolution of ascertaining whether Government will undertake the publication of Mr Sang's valuable tables, —for the cost would go far beyond our own resources as a Society.

2. I cannot conclude what I have to say of our Society, without adverting to the losses we have sustained by the death of several distinguished Honorary Associates, viz., Louis Agassiz, Lambert Adolphe Jacques Quetelet, Auguste de la Rive, and John Stuart Mill. Obituary notices of some of these distinguished men have already appeared in our Proceedings. The others will be noticed by our President in his Address, when the next session commences.

The Council have filled up these vacancies by selecting other eminent men as Honorary Associates; and these nominations have been confirmed by the Society.

Whilst referring to the list of our Honorary Fellows, I cannot avoid mentioning a name, which I see standing on the same page, and standing by itself, viz., *Sir Richard Griffiths.*

Sir Richard is, I believe, the oldest member of our Society. He will, in September next, have completed his ninetieth year. He was in my house two months ago, in good health, on his way to Ireland, where he is at present residing. It so happens that this forenoon I had the pleasure of receiving a letter from him, accompanied by a short biographical memoir of his many scientific researches as a geologist, and of his great public services as a high officer of the Irish Government.

This memoir I have brought with me this evening, that you may see in it an excellent photograph of my venerable friend, and our oldest colleague.

III. Having said all that occurs to me of ourselves and our own doings as a Society, perhaps you will permit me, before closing, to allude to what is doing generally in the country for the advancement of science.

There are two aspects in which science may be viewed:—*First*, The teaching of what is known; and, *second*, The investigation of what is not known.

1. As regards the teaching of what is known,—

(1.) I must advert to the great additional impulse lately given in Scotland, England,and Ireland, by the Kensington Department of Science and Art.

The efforts of that department are confined chiefly to schools—Middle-Class Schools—though they do not object to assist science classes, even in elementary schools.

In Scotland, ten years ago, there were only 4 schools in connection with the department, now there are 118. I observe a list of 42 of these Scotch schools to which payments were made last year, amounting to L.1746, as perquisites to the teachers, which is no small pecuniary encouragement.

(2.) Another measure—not yet adopted, but which, if adopted, will probably conduce to the advancement of science teaching—is one about to be proposed to Parliament by a distinguished Fellow of the Society, Mr Lyon Playfair. He intends to ask the House of Commons to pass a resolution, recommending the Government to create a department for Education, Science, and Art, with a responsible minister at the head of that department.

I feel very sure that Mr Playfair will be able to make out a good case for such an appointment.

There is no country in the world whose various industries are more benefited than ours, by the help of science, and by that official encouragement of science which is at present almost entirely awanting.

2. The other aspect of science to which I referred, is aiding in the investigation of truths not yet known.

In Germany there exist colleges of research, in which persons can get the use of instruments, and a laboratory for conducting experiments in any department of science to which they devote themselves.

Every one in the least acquainted with science must appreciate the importance of such institutions. In this country there are none, at least none except on a very small scale, and belonging to individual professors. But I am happy to say that there seems every prospect of this great desideratum being likely to be soon supplied.

The need of these institutions has been for some years pointed out by various scientific bodies, ourselves among the number; and the fruits of these expositions are now appearing.

(1.) Thus, I read in the "Scotsman" newspaper of Saturday last, that in *Glasgow* University such an institution is about to be formed. Principal Caird announces it in these terms,—terms which, whilst gratifying to Glasgow University, you will see that we as a Society also have reason to be proud of.

" A valuable gift has been made to the University by one of its most distinguished professors, Sir William Thomson, in conjunction with the representatives of Dr Neil Arnott. To a donation of L.1000 by Dr Arnott's widow, Sir William has added a sum of L.2000 for the endowment of the new office of Demonstrator in Experimental Physics, in connection with the chair of Natural Philosophy. By that endowment Sir William has conferred new obligations on the University, on which his great name reflects so much honour."—*Scotsman Newspaper*, 2*d May* 1874.

(2.) Another example I find from the *April* number of "Nature," in the following terms:—

" The magnificent sum of L.10,000 has been made over by the late Mr E. R. Langworthy to the Owens College, Manchester, for the purpose of developing the chair of Experimental Physics. The terms in which the bequest is made are so forcible and clear, that they deserve to be quoted:—' I bequeath to the trustees of Owens College ten thousand pounds. It is my desire that students may be instructed in the method of Experiment and Research, and that science may be advanced by original investigation. I also desire that the professor appointed may be selected on account of his knowledge having been especially obtained by original investigation, and that his appointment shall be contingent upon the continuance of such investigation.' "

(3.) The third example I give is from the *March* number of the same newspaper.

" The Cavendish Laboratory.—This laboratory, in which every facility is furnished for the prosecution of physical research, is the munificent gift of William Cavendish, Duke of Devonshire, K.G., Chancellor of the University of Cambridge, who has intimated his intention of presenting it complete to the University.

" The building, which is now finished, was erected at an expense of about L.10,000.

" The laboratory is open daily from 10 A.M. till 6 P.M., under the superintendence of the professor of Experimental Physics, for the use of any member of the University who may desire to acquire a knowledge of experimental methods, or to take part in physical researches."

3. These measures are all intended to give greater facilities for original research. They indicate the strong belief existing in all thoughtful minds, of the importance of giving such facilities.

It is to be wished that this opinion may have impressed itself on the minds of the Royal Commissioners, who are about to issue their report on the aid which should be given in this country by the State to science.

It is a hopeful circumstance towards that view, that His Grace the Duke of Devonshire, to whom I have just alluded, is at the head of that Royal Commission. His munificent gift to Cambridge University shows how well His Grace knows what is necessary for the advancement of science in this country.

IV. With these remarks, gentlemen, I now declare the Winter Session of our Royal Society closed; and I only farther express a hope that all of us here may be spared, and many more of our colleagues also, to meet again at the commencement of our next Winter Session.

The following Gentlemen were elected Fellows of the Society:—

JOHN CHIENE, M.D., F.R.C.S.E.
JOSEPH BELL, M.D., F.R.C.S.E.
E. A. LETTS, Ph.D., Assistant to the Professor of Chemistry in the University of Edinburgh.
BADEN POWELL, Esq., Conservator of Forests in the Punjab.

Donations to the Royal Society Library during Session 1873-74 :—

I. Authors.

Archer (William Henry). Abstracts of English and Colonial Patent Specifications relating to the Preservation of Food. Melbourne, 1870. 8vo.—*From the Author.*

Arrest (Dr H. d'). Indbydelsesskrift til Kjbenhavn Universitets Aarsfest til brindring om Kirkens Reformation. 4to. 1872. —*From the Author.*

Beetz (W.). Der Arnheil der K. bayerischen Akademie der Wissenschaften an der Entwickelung der Electricitätslehre. Munich, 1873. 4to.—*From the Author.*

Benson (Lawrence S.). Notes on the First Book of Benson's Geometry. 1873. 8vo.—*From the Author.*

Bow (Robert H.), C.E. Economics of Construction in Relation to Framed Structures. 1873. 8vo.—*From the Author.*

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Döllinger [(J. von). Rede in der Öffentlichen Sitzung der K. Akademie der Wissenschaften am 25 Juli 1873. Munich, 1873. 4to.—*From the Author.*

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II. TRANSACTIONS AND PROCEEDINGS OF LEARNED SOCIETIES, ACADEMIES, AND UNIVERSITIES.

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Annual Reports of the State Engineer and Surveyor of the State of New York, 1869, 1870, 1871, 1872. 8vo.—*From Sir Charles Hartley.*

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Verhandelingen dér Koninklijke Akademie van Wetenschappen. Deel XIII. 4to.—*From the Academy.*

Amsterdam.—Verslagen en Mededeelingen der Koninklijke Akademie van Wetenschappen Afdeeling Natuurkunde. Deel VII. Letterkunde, Deel III. 8vo.—*From the Academy.*

Basel.—Verhandlungen der Naturforschenden Gesellschaft. Theil VI. Heft 1. 8vo.—*From the Society.*

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Die Fortschritte du Physik im Jahre 1869. Dargestellt von der Physikalischen Gesellschaft zu Berlin. XXV. Jahrgang, 1 und 2 Abtheilung. 8vo. — *From the Society.*

Jahresbericht der Commission zur Wissenschaftlichen Untersuchung der Deutschen Meere in Kiel für das Jahr 1871. Fol.—*From the Commission.*

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